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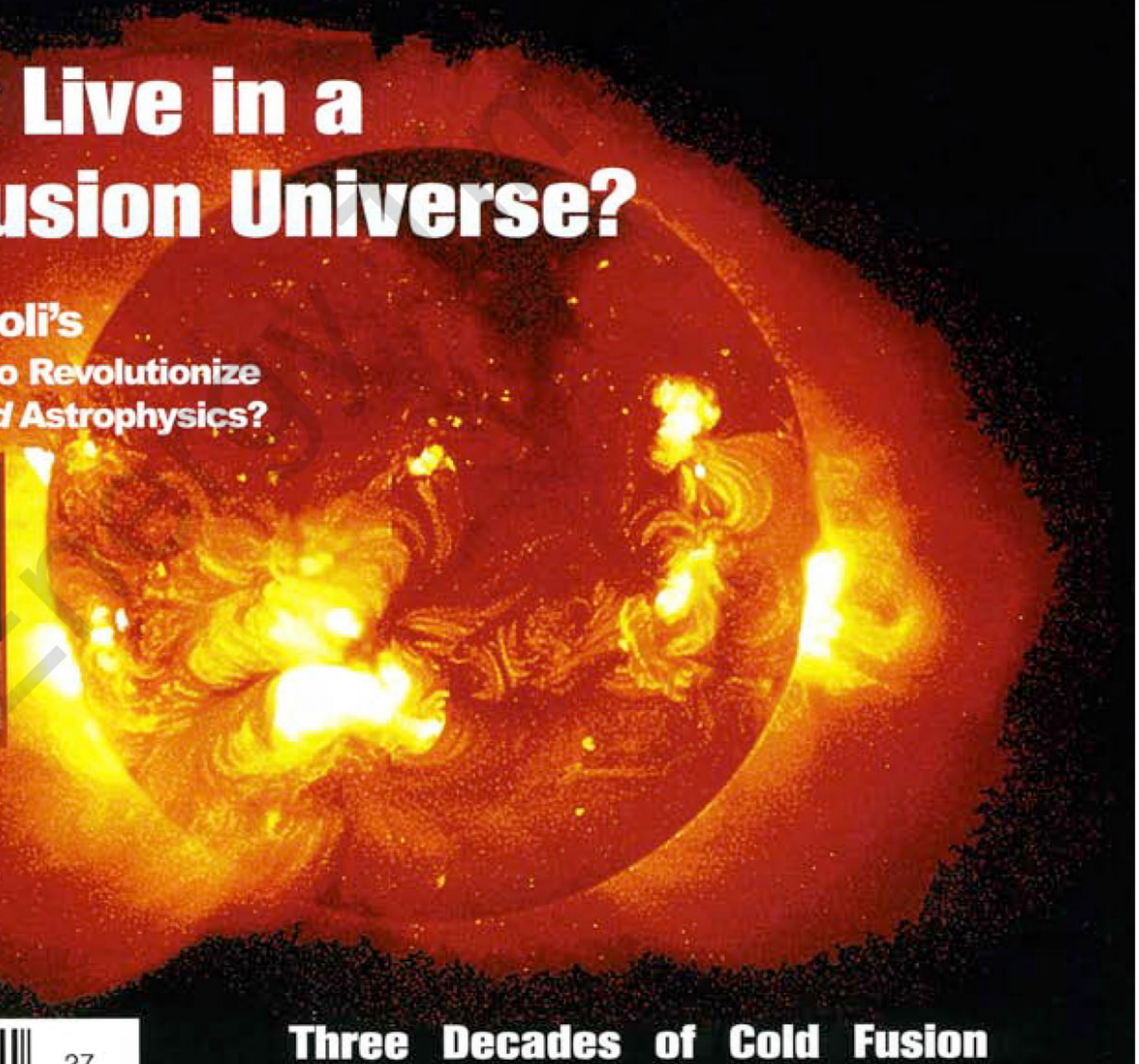

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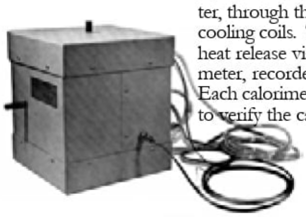

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Infinite Energy
Cold Fusion and New Energy Technology

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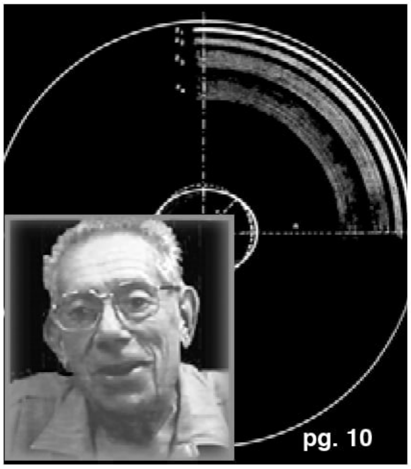

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Cold Fusion & New Energy Lectures
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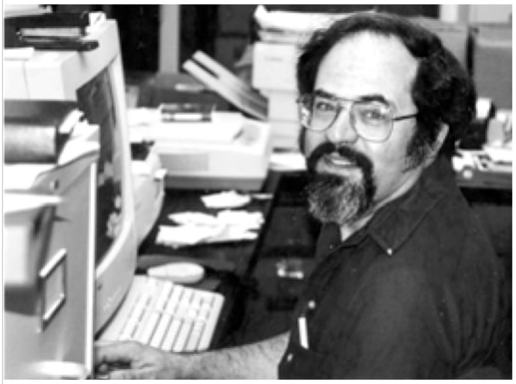

By Eugene F. Mallove, Sc.D.

Expectation is in the air. As the world makes its transition to a new century, will the embattled cold fusion (low-energy nuclear reactions, LENR) field succeed in convincing humanity that it is for real—that it will be a major factor, perhaps *the* most important factor—in ending the Hydrocarbon Fuel Age? We fervently hope so. We occasionally despair that it may take a proverbial "miracle" to make that happen, so bereft of adequate resources is this embattled field.

Admittedly no utilitarian (scaled-up) cold fusion heaters or electric generators have yet been marketed. This the critics of cold fusion use against us with a vengeance, though that is really a measure of their desperation. They had predicted that the cold fusion field would wither away. It hasn't, it won't, and they know it. This makes them very upset.

Of course, it is science turned on its head to say that commercial products on the market are needed for *scientific* validation. Since when should the lack of a viable commercial product be an indicator of the reality or future potential of a newly-discovered physical phenomenon or technological achievement?

A one-hour moderated debate on the Los Angeles public radio affiliate station that I had on July 7 with Prof. Robert L. Park is instructive. Park, as many *Infinite Energy* readers know, is the *de facto* spokesman of the American Physical Society(APS) in his weekly *What's New* cyber-column; he is no friend of cold fusion. From the outset in 1989, Park has been disparaging cold fusion research as incompetence or fraud—take your pick.

On the recent program, which was kindly arranged and moderated by Michael Shermer of *Skeptic* magazine, I reminded Park and the radio audience of what he had said back in 1989 and in 1991. In the *Chronicle of Higher Education* (June 14, 1989) he said: "The most frustrating aspect of this controversy is that it could have been settled weeks ago. If fusion occurs at the level that the two scientists claim, then helium, the end product of fusion, must be present in the used palladium cathodes." In the spring of 1991, when I telephone interviewed him for a comment to be included in my then forthcoming book *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor* (John Wiley & Sons, 1991), Park remarked: "You don't have to worry about the [excess] heat if there is no helium."

Today in the peer-reviewed scientific literature there is copious evidence for this helium product in association with nuclear magnitude excess heat. In some data there is nearly perfect commensurate correlation of heat and helium product. Does this move Professor Park to change his stance? Not on your life! On this summer's radio program he talked completely around the helium issue, blathering about the absence of commercial products as evidence that this field is not for real. He has the data in hand; he is aware of it; and he has heard the case made by the original experimenters. He has no excuse. Certainly in this matter Park is not acting as a scientist, rather a propagandist for status quo physics—"don't bother me with the experimental facts" fizzix.

The symptoms of a scientific community at war with Mother Nature run broad and deep. Look at this month's "Briefs," the story about organized attempts to strip Prof. George Miley of his fairly awarded DOE Nuclear Energy Research Initiative (NERI) grant in low-energy nuclear reactions, specifically aimed at nuclear waste remediation. Late breaking news about this bizarre episode is that the unnamed DOE "powers that be" have augmented (packed?) with three more reviewers the original three-member panel—charged with assessing the science behind the NERI Miley grant. One of these is none other than Dr. John Huizenga, the former co-chairman of the 1989 ERAB Cold Fusion Panel, also author of *Cold Fusion: The Scientific Fiasco of the Century* (1992). To say the least, with Huizenga aboard, Miley's prospects can't be very good. But we shall see.

One more symptom of ill winds blowing against cold fusion: The American Chemical Society (ACS) 1999 Pacific Conference on Chemistry and Spectroscopy (October 6-8, 1999) is scheduled to have an Electrochemistry session (Friday, October 6), organized by electrochemist and cold fusion scientist Dr. Melvin H. Miles of the U.S. Naval Air Warfare Center at China Lake. In addition to conventional battery technology discussions, talks had been arranged and put on the program to be delivered by cold fusion luminaries Drs. Martin Fleischmann, Ed Storms, Michael McKubre, Stan Szpak, John Dash, and Robert T. Bush.

It transpired that "numerous" ACS members called in to protest this electrochemistry session because of this cold fusion component. There apparently was an organized move for mass resignations from the ACS if this cold fusion session was not quickly taken off the program. Not only that, but chemistry Nobel laureate (1985) Jerome Karle, who was scheduled to be a keynote speaker at the meeting, made it clear to the meeting organizers and the ACS that he would cancel his talk if the cold fusion session was allowed to go forward. Sigh—yet another devaluation of the integrity of the science Nobel prizes.

And so the scene is set: "Pharaoh's army" is chasing the cold fusion rebels on their way to the Promised Land of open science and perhaps infinite, clean energy. Will the seas part and the beleaguered "Israelites" of cold fusion escape? Will Pharaoh's minions be drowned in the very medium whose potency this army so hotly disputes? Will a "miracle" happen? A "small miracle" would do just fine, thank you.

How about a big one. . . ? One *may* have arrived just in time for the impending millennial festivities. We draw your attention to our cover story about Signor Renzo Boscoli's work in Italy. It apparently long-predates the Fleischmann-Pons era in its conception and even in its experimental embodiment. It is cold fusion, warm fusion, low-energy nuclear reactions—whatever you want to call it, but it is *very hot* in more ways than one. Sometime in October 1999 the necessary details will all be out in the public arena, posted to our website www.infinite-energy.com, waiting for worldwide replications to occur (unless a hang-up occurs at the European Patent Office). We have satisfied ourselves that these replications are likely to happen.

If these do occur, the worldwide shock waves could make the 1989 Utah announcement look like a mere warm-up event. This is a high-temperature solid-state fusion process that appears to be virtually universally applicable to energy systems—from planes, trains, automobiles, houses, to spaceships. It is not quite as squeaky clean as the gentle bubbling of an electrochemical cell or the catalytic production of helium and excess heat. The Boscoli process exceeds 1,000°C within *seconds* of triggering and it is said to be 100% reproducible. For those skeptics, perhaps even those in the cold fusion field who may feel threatened by this new development, we fully agree that *the proof will be in the testing.*

So go to it men and women, prepare for the October Surprise! The process is said to be simple and even long-term, self-sustaining, as long as it is fed with the requisite deuterium fuel.

The arrival of this process in its full-blown replicated form could be the Biblical equivalent of the parting of the Red Sea. But just remember, we already have enough cold fusion "boats" to get us across that angry sea and away from Pharaoh's troops. These little boats— the little miracles that we are already seeing in laboratories around the world—will also take us safely to the Promised Land, albeit with a few more years of journeying. If Boscoli's process should prove itself, we are probably home free. In the coming weeks, remember to think positively: "Miracles" Happen.

∞

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LETTERS TO THE EDITOR

Cold Fusion Will Replace Oil

A recent article in *Home Power* magazine reports the world will run out of petroleum by 2040. At some point between 2010 and 2025, the price of transportation fuel in the United States will be too expensive for the average U.S. consumer to afford.

Adding to the problem will be the depletion of natural gas reserves. The *Home Power* magazine article says cheap natural gas reserves will soon be gone. That could put millions of U.S. citizens at risk of freezing to death in the winter.

Things must change. I will soon begin working for a company that manufactures fireplace inserts, wood stoves, and furnaces. New technology breakthroughs will help. We will need rapid continuous improvement and innovation in heating homes and businesses.

Enclosed is $34.95 for one copy of the new video, "Cold Fusion: Fire from Water." This new technology may help heat our homes and run our motor vehicles.

Donald Dyall
Mt. Pleasant, Iowa

* * *

Skeptics Belong in Medieval Times

Recently I ordered two copies of your new video documentary, "Cold Fusion: Fire from Water." One word: AWESOME. I was very, very impressed. The overall technical quality is very high, of course, but the informational content is nothing less than stunning. If this videotape isn't able to convert a critic to, at least, a listener, I really wouldn't know what would.

I have sent the other videotape to a friend of mine, who is working for NASA. Actually, I know for a fact that he too will be impressed with this video. And I'm equally sure that he will check this out thoroughly—probably he will show it to some open-minded scientists at NASA as well. I sent him a couple of books on this topic also.

Personally, I feel this is very important. This information is so incredibly important, so crucial, it must be shown to everybody, at least anybody who may have the means and contacts to change something about all this. All efforts should now be focused on broadcasting this tape worldwide—on every channel available.

The way these courageous and brilliant researchers are treated, in fact, the way anybody is treated who has the nerve to use the words "cold fusion," is outrageous. The arguments of the "opposition" have nothing to do with science—these are the techniques and manners used by the inquisition in dark and medieval days.

In my opinion, Pons and Fleischmann should be nominated for the Nobel Prize in electrochemistry and physics—pronto.

I congratulate you with a job extremely well done. You have a fine, high-quality magazine also. I am not an expert in this field, but thanks to the excellent style and content I enjoy your magazine every time, learning and understanding from each and every article.

Keep up the good work! We never met, sir, but you have my deepest respect.

Andre La Pierre
Holland

* * *

IE "Meaty," But Good For You

I would like to thank you for the very prompt and very forthright answer to my letter of inquiry to you.

On Friday, I had the secretary in the office subscribe me to your magazine, *Infinite Energy*. To my amazement, I received the first issue of my subscription on Tuesday. Very impressive! I belong to several book clubs, mostly of a scientific nature, and have never gotten such prompt service.

I am also impressed with the contents of your magazine. Of course, I have only had a chance to skim through it, because it is quite "meaty." It will take me a while to read all of it. I especially like the feature of being able to order back issues of your magazine. Within the next few days, I am going to order eight of the back issues.

In your letter, you mentioned J.L. Naudin's progress with a motor, using the Newman technology. While I do agree that he is much more scientific in his approach, especially with documentation, I have a problem with the fact that he is using external coils and capacitors. This is, in effect, an LC resonant tank circuit. This could be what is required to take a Newman motor over-unity. However, anytime you incorporate an oscillator into the circuit, you have to accurately compute what the resonant frequency is, and more importantly, what the decay rate is for the tank. This has to be done prior to installing it into the system. I have seen no information in this regard. Furthermore, exact coil size and capacitor size is not given. Without this information, I cannot mathematically compute a formula to either prove or disprove Mr. Naudin's claims. I am not finding fault with his design, only disappointed that there is not more information in regards to his methods.

I personally feel that one person who may have the answer to the Newman technology is Stefan Hartmann. He is very thorough and documents everything. In addition, he brings the German concept into it (thoroughness, precision, meticulous documentation, and pride in workmanship).

I am also working on a prototype. Since I am employed by an electric motor shop, I have everything I need at my disposal, including the owner's blessing. (He paid for my subscription to *Infinite Energy* Magazine.) I have designed this prototype as a sealed unit, with the ability to incorporate liquid nitrogen into the motor. I will be able to test it under several conditions and in all environments. The armature is being wound this week and, hopefully, assembly and testing will take place next week. All this is dependent on my work load. I will keep you informed as my progress develops.

Norm Biss
Erie, Pennsylvania

* * *

Comments on Lightning (*IE* No. 25)

I greatly enjoyed Professor Graneau's article, "Why Does Lightning Explode and Generate MHD Power," in Issue No. 25 of *IE*. He proposed that the energy to produce thunder may be chemical bond energy stored in nitrogen and oxygen molecules of the air.

Water molecules and high voltage are present when thunder is produced. Dr. Randell Mills claims to have produced fantastic amounts of energy by transferring hydrogen atoms into lower energy states which he calls hydrinos (*IE*, Issue No. 17, pp. 21-35).

I have claimed that many experimenters (some extremely well-respected) have produced hydrogen in and from vacuum (*IE*, Issue No. 17, pp. 79-85). All of these experiments consumed large amounts of energy.

The above leads me to propose that the energy which produces thunder comes from conversion of hydrogen atoms in water into a lower energy state.

Water and high voltage are present in Graneau's MHD (magneto-hydrodynamic) experiments, so the energy he observes in these experiments may be produced similarly.

Ball lightning, which is accepted as a real phenomenon, is quite rare and of short duration. It ends either quietly or in an explosion. I propose that the conditions of voltage and water concentration required for ball lightning are very exacting. The ball lightning persists only as long as an equilibrium exists. A slight change either causes the ball to fade away or leads to a chain reaction and explosion.

Paul E. Rowe
Mashpee, Massachusetts

* * *

Little black holes may produce phenomena similar to ball lightning, but it is not a requirement. In fact, I fully agree that if an LBH were to enter an atmosphere it would exhibit characteristics similar to those given in the article. I also agree that it should pass by "normal" matter because the event horizon is too small to effect "normal" matter, not to mention forces due to its radiation. Of course, this depends on whether the predictions of Steven Hawking *et al.* are correct.

Clint Seward and others have demonstrated in the laboratory that ball lightning is most likely a spinning toroid. The toroid is initiated by the intense potentials and fields at the moment of a lightning bolt combined with the tangential intersection of electron current in another bolt of lightning. The interaction of the intense potentials causes the second electron current to form a stable toroid. It may spin due to external influences and it will ionize the surrounding air, appearing as a ball. Clint or one of the other researchers may want to publish from their point of view.

Why they last so long is a good question! The electrons may form pseudo-atomic structures, *i.e.* electron clusters, that may not be required to radiate synchrotron radiation. Many questions remain.

David B. Hamilton, U.S. DOE
Washington, DC

* * *

Cold Fusioneers Own Worst Enemy?

Thank you for your letter of May 24 and the videotape, which I have now had time to view. I'm afraid both the letter and the video tend to confirm my sense that the cold fusion community is its own worst enemy.

First the video (I speak now not only as a physicist, but also as an experienced television hand—I produced a substantial series of programs for PBS during the 1980s): Why does Cold Fusion want to be associated with "Star Trek?" Do you think Cold Fusion conferences resemble "Star Trek" conventions, or do you just want to suggest that cold fusion supporters are as nutty as Trekkies? In any case, the actor is painfully out of his depth and certainly not an asset. And, why is the program seventy minutes long? That effectively precludes it from broadcast television. Who makes such decisions?

In your letter you call me a critic of cold fusion. If you mean that in the sense that scientists should always think critically, then I would agree. But I don't think that is what you mean. What you mean is that I'm a member of the enemy camp. In the siege mentality of the cold fusion people there are only two possibilities, and you are of course right in thinking that I am not an uncritical supporter of cold fusion. In fact, the article I wrote was designed to be the beginning of a bridge between cold fusion and mainstream science, because I believe there are experimental results that deserve serious consideration, and would get it had not Fleischmann and Pons done their horrible damage. But because the article is not uncritical, the response of the cold fusion supporters has been mostly vitriolic abuse (your letter is more polite).

Nonetheless, I will continue to take some interest in what happens in your field, and I have been giving Franco Scaramuzzi some assistance in preparing for ICCF-8.

David L. Goodstein, Vice Provost
California Institute of Technology
Pasadena, California

[Dr. Mallove Responds:] I was very happy to receive your response to my letter and a reaction to our new video, "Cold Fusion: Fire from Water." I admire your excellent physics television series, "The Mechanical Universe," which I had the occasion to use in teaching an introductory physics course.

Your objection to our using James "Scotty" Doohan of "Star Trek" fame puzzles me. This was merely an attempt to have a well-known narrator whose character is well-respected by many in the technical community and widely known by laypersons. I do not think Doohan is "painfully out of his depth," and we certainly did not intend to suggest anything negative about the cold fusion community—this is more the habit of your Caltech colleagues Professors Koonin and Nathan Lewis and certain recalcitrant figures at MIT. (You should know that Dr. Koonin returned *his* tape, apparently unwatched.) On another point, fear not, the seventy-minute version we sent you is for home video use or local cable TV use. We have a fifty-six-minute version ready for broadcast by any PBS affiliate that wishes to do so. Discussions are underway with several national networks that may eventually broadcast the program.

I was thrilled to learn that you are helping Prof. Francesco Scaramuzzi organize the ICCF-8 cold fusion conference in Italy, which is to be held in May 2000. I know that you mention Francesco favorably in the famous cold fusion essay posted on your website. Perhaps by coming to ICCF-8—and I most certainly *do* encourage you to attend—you will learn that cold fusion conferences are not much different than other serious scientific meetings. I have been to all but ICCF-2 and I have missed seeing you at any of the others. Attending ICCF-8 might lead to a beneficial first-hand update of your essay.

Yes, you most certainly are a critic, though not firmly in the "enemy camp"—most certainly there is such a camp (just read the stuff that APS's Robert L. Park purveys and you'll get an idea of the belief system of that group). You say that your article "was designed to be the beginning of a bridge between cold fusion and mainstream science, because I believe there are experimental results that deserve serious consideration. . ." Very well, we agree on the need for that bridge. But then you go on to say that cold fusion would have had a better chance "had not Fleischmann and Pons done their horrible damage." I beg to differ with you.

Though there were problems in the beginning due to the manner of the announcement and so forth, which was not by any means under the complete control of Drs. Pons and Fleischmann, the basic problem was the smear campaign against cold fusion—then and continuing. Despite mounting positive evidence early on, there were those who preferred a rush-to-judgement with a negative conclusion, *i.e.* the Close, Taubes, Huizenga line. I respectfully suggest that you consider the harm done by the wrongs of these extreme critics of cold fusion. Their deeds and words we have amply documented.

Think about this too, just as an example that happens to be close to your home: Consider the harm to openness in science that came from the rejection of several submitted Letter-to-the-Editor critiques of Prof. Lewis's cold fusion calorimetry work published in *Nature*. These were rejected out of hand by *Nature*, using Lewis himself as the "peer reviewer"! These critiques were later published in other peer-reviewed journals, albeit more obscure ones. That is just one example of many unfortunate occurrences.

One of the saddest realizations for me is that the evidence in favor of the reality of low-energy nuclear reactions has long been established (as far as I am concerned), yet it will definitely require commercial devices on the market to turn around the tide of opposition. This is science turned on its head. On the latter point I think we could both agree.— *EFM*

* * *

BYU Has Not Withdrawn Claims

While I enjoyed our recent telephone conversation, I was concerned by your remark that many people evidently assume that we at Brigham Young University have withdrawn all of our anomalous neutron-production claims. This is not the case.

In *Trans. of Fusion Technology*, 26:143-148 (December 1994), we reported on "large bursts of neutrons (over 60 neutrons, in a 320-microsecond window) whose signals unrealistically came from just one quadrant" and we retracted claims of such large "bursts," as defined above. Such "high-multiplicity bursts" in very short time intervals were generated in the detector used in later experiments, and were traced to "high-voltage breakdown in the standard electronics of this ^{3}He-type detector" (*Ibid.*, p. 144).

This is the kind of detector which we used in collaborative experiments at Los Alamos. This is not the kind of detector used in the original BYU experiments, which are based on use of scintillators and photo-multiplier tubes. We began using the latter detectors at BYU in 1986 for "cold fusion" experiments, and these have never shown such high-voltage breakdown problems. We have not retracted the results from the BYU neutron spectrometer as published in *Nature* 338 (April 1989) 737-740. Nor have we retracted the other (non-ultra-short-burst-type) results obtained in the 3He-type detectors (*e.g.*, Menlove *et al.*, *Journal of Fusion Energy* 9, 1990, 495-506).

The first publication regarding our piezonuclear fusion research was published in March 1986 in a paper entitled "Piezonuclear Fusion in Isotopic Hydrogen Molecules" (*Journal of Physics G. Nuclear Physics* 12 (1986) 213-221.). This was authored by Clint Van Siclen and myself and submitted in 1985. We began "piezonuclear" experiments in May 1986 at BYU, and such experiments continue at BYU to this day. Elsewhere very similar research continues under the name "pycnonuclear fusion." One may call this "cold fusion" if he/she wishes (BYU Prof. Paul Palmer used that term for our work at BYU back in 1986), but now the term has been applied to so many different claims that it no longer conveys the intent of our pioneering 1985 paper. Certainly our results do not support the claims of "excess heat" production via nuclear reactions in electrolytic cells made by others.

As Dr. Fleischmann commented years ago (I believe in 1989), if the "excess heat" he claimed is real, it is theirs — but if the energetic nuclear particles we claimed are real, these are ours (at BYU). I can agree with that. The final chapter has yet to be written. In particular at BYU, we remain interested in the possibility of geological fusion as described in our April 1989 paper in *Nature*.

My best regards to those researchers who are open-minded and who seek compelling evidence using different detectors whose signals agree quantitatively and are many times the background levels in all detectors.

Steven E. Jones
Provo, UT

[Dr. Mallove Responds:] Thank you for your very fine letter of clarification about your publishing record in "pycnonuclear fusion" or "cold fusion." I do believe that there has been some confusion about your present opinions about your past work. Our readers will be happy to know that you still stand by it. I am especially glad to learn that you remain interested in geological fusion as described in your 1989 *Nature* paper. I trust that Dr. Petrasso of the MIT-PFC continues to disagree with your views on this, perhaps preferring to believe in his famous "nearby nuclear submarine" explanation for the elevated tritium in volcanic emissions (*cf.* his statements at ICCF-1, 1989). I am glad that you remain supportive of those who seek compelling nuclear radiation evidence, but may I suggest that you take a good hard look at the helium evolution data being seen in the Arata-Zhang experiments in Japan and in the catalytic fusion work of Dr. Les Case, which has now been confirmed at SRI International.—*EFM*

* * *

Errata (*IE* No. 26):

On page 13, the picture caption should have read Dr. Edmund Storms, not Dr. Edward Storms.

On page 14 it was incorrectly stated that the "sun is about eight minutes behind its real instantaneous position." It should have read, "The position in the sky where we see the sun is about 20 arc seconds behind its real instantaneous position, yet the Earth in its orbit responds to the Sun's real-time position."

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Beyond the Cutting Edge

Jeffery D. Kooistra

DON'T BURN THOSE BOOKS!

"When an ordinary scientist finds something weird, he shies away from it. The great ones chase the weirdness."

Anonymous

Some *Infinite Energy* readers are familiar with the column I write for *Analog Science Fiction and Fact* called "The Alternate View." In a recent column I briefly discussed an enigmatic effect first described in the literature by Dr. Peter Graneau in a letter called "The Missing Magnetic Force Law" (*Galilean Electrodynamics* Vol. 9, No. 2, March/April 1998, pp. 35-36). This effect is the retrograde motion (that is, toward the breech) of a ferromagnetizable armature on a primitive railgun. The Lorentz force law, of course, predicts only motion toward the muzzle.

A friend tried the experiment and sent me a letter about it. Since his letter describes clearly both his experimental method and his results, I'm reprinting it here:

> Jeff,
>
> I know you are always a careful experimenter, but I have to admit that I thought you were bonkers when I read your latest *Analog* article this afternoon. I immediately drove to my father-in-law's house and pulled out two saw horses (decorated to look like sheep from last Christmas' church play); two 1/2 inch grounding rods, 8 feet long; one two-foot length of 3/4 inch copper pipe; one two-foot length of black pipe (which is steel pipe with no galvanizing for gas lines); a four foot carpenter's level; and a steel brush.
>
> From the garage I grabbed a pair of jumper cables and a garden hose with a spray handle on the end and connected the hose to an outside spigot.
>
> I set up the saw horses approximately four feet apart and leveled them with respect to each other by adjusting the separation of the legs. I laid the two grounding rods across the saw horses approximately one foot apart. I energetically removed the oxide layer from the rods, and both pieces of pipe with the steel brush. I drove my Bronco next to the arrangement and connected the jumper cables to the battery. I connected the positive clamp to the end of one of the rods. I then liberally sprayed the rods and the copper pipe with water from the garden hose (I figured any temptation on the part of the rods to spot-weld together would be suppressed by the energy required to boil away a few grams of water first) and laid the pipe across the rods.
>
> I clamped the negative jumpercable wire to the end of the other rod and smiled in satisfaction as the copper pipe sparked and sputtered while it rolled slowly away from the jumpercable clamps. It picked up speed until it reached a section of the rods which I had not cleaned and there it spot-welded itself to the rods.
>
> Removing the negative clamp and the pipe from the rods, I then recharged the battery with my 351cid battery charger, sprayed down the steel pipe, and laid it across the rods. I reconnected the negative clamp and remember seeing sparks leap, not only from the contacts between the rods and the pipe, but from my own brain! The pipe accelerated smoothly and rapidly (much more rapidly than did the copper pipe) the WRONG WAY!
>
> I repeated the experiment several times before locating and burning all of my physics textbooks.
>
> Regards, P. Scott DeVos

Given the title of this column, I must say immediately that I know Scott was only speaking rhetorically—he didn't burn all of his old physics textbooks.

Yet some frontier scientists do act as if they've gotten rid of all their books. Yes, as many of us have come to discover, to our own chagrin, "the book can be wrong." But one must be careful to distinguish just what it is the book is wrong about.

Ed Wall and I have been doing a great deal of retrograde railgun work here in the lab lately. Our first cut at analyzing our peculiar results involves trying to apply basic right-hand-rule electromagnetism to the situation. Of course, we learned these rules from our physics books and physics instructors.

It is important to remember that not all textbooks are created equal. Some are excellent, clearly written, and much more thorough than others. They may contain answers to questions that appear as mysteries in lesser textbooks.

Some texts have a different "take" on things than do others. For instance, there are several physics "bibles" on the subject of gravity. One is *Gravitation* by Misner, Thorne, and Wheeler. Another is *Gravitation and Spacetime* by Ohanian. If you were trying to figure out how Mach's principle "fits in" to modern thinking about gravity, you'd get very different impressions from each of these texts.

There is most definitely only one physics that the Universe plays by—but there is not only one physics in our textbooks. There are many variants, the variations depending not only on copyright date, but on geography as well. It is only when one has delved deeply into many different fundamental texts that one can genuinely appreciate the diversity of opinion.

Returning to the retrograde railgun, it is becoming clear to me that no "new physics" is required to understand it. But in this context, "new physics" means some new physical principle. There is, however, a great deal of poorly explored physics at work in the retrograde railgun, where the approximations we take for granted in most work have to be thrown out and the much harder task of working with the full, non-truncated physics has to be applied.

And some of this poorly explored physics is so little understood that the area might just as well be encaptioned "here be dragons" on the map of the world beyond the cutting edge.

❑❑❑

"Warm Fusion" Experiments and Theory by Signor Renzo Boscoli of Italy Predate the Fleischmann-Pons Era

Reproducible, High-Temperature Cold Fusion Experiments to Be Revealed in October 1999 International PCT Patent Office Application

Eugene F. Mallove

Do We Live in a Cold Fusion Universe?

. . . might well be the question to ponder when reading the two articles by Italian scientist Signor (Mr.) Renzo Boscoli that follow this introduction. These chronicle Mr. Boscoli's attempts to find a more satisfactory model of stellar fusion processes—one that could account for several questions that even contemporary astrophysicists agree continue to plague stellar physics. The "king" of such mysteries is the "missing solar neutrino" problem, which is now generally accepted as an experimental fact.[1,2,3] Correct or not, Renzo Boscoli makes the astonishing claim, in fascinating and very eloquent arguments, that the Sun works by what would normally be called *cold fusion* reactions, rather than by what the reigning paradigm suggests: multi-million degree thermonuclear fusion (for which the 1967 Nobel prize in physics was awarded to Hans A. Bethe "for his contributions to the theory of nuclear reactions, especially his discoveries concerning the energy production in stars").

To be sure, virtually all people in the cold fusion field, including this Editor, have not heretofore even *thought* of questioning that stars are celestial thermonuclear reactors—the orbs that controlled-fusion folks were trying to mimic here on Earth. After all, we have always known that astronomers have labored long and hard to work out a detailed evolutionary history of stars both on and off the "main sequence" of initial composition. These observations and patterns were predicated, it would seem, on carefully considered astrophysical models of stellar nuclear reactions and structure.[4] (To be precise, Dr. Randell Mills of BlackLight Power Corporation—see his interview in *IE* No. 17—has proposed that a good part of the Sun's power is, in fact, generated by the catalytic lowering of hydrogen electron energy levels to below the conventionally understood ground state.)

Temporarily suspend your disbelief, but please *do* read these fascinating articles by Boscoli: "Note on 'Thermo' Nuclear Fusion," which he wrote in June 1984 and his October 1992 "Update." After reading these, *forget* the stars and what nuclear processes may truly be at work in their nuclear cores. It could very well be a moot point whether models of stellar structure need drastic revision to accommodate Mr. Boscoli's theories of low-energy nuclear reactions. *Inspired by his astrophysical reasoning*, this Italian scientist of considerable accomplishment (see his brief biographical summary) has been quietly *experimenting* in Italy on cold fusion processes since the late 1970s—years before the Fleischmann-Pons experiments that led to the March 23, 1989 announcement at the University of Utah. The most remarkable fact about this work is that Boscoli's physics experiments on Earth were based on inferences from an extremely heretical astrophysical model—a Sun with a core of ultra-cold, solid material in and on which low-energy nuclear reactions occur. These create the approximately 5,800°K solar surface high above this reacting core. In a classic model of science, a theoretical prediction led to an experimental result that *worked*—leading apparently to a process with enormous technological implications.

Why have we not heard of this work before? It must be said that Boscoli, to maintain confidentiality, deliberately avoided attending cold fusion conferences. An associate of his did so for many years, which is how this article came to be. Boscoli has been aware of and saddened by the persecution that his cold fusion brethren were undergoing. He wrote in his 1992 article, "Demonstrating the zenith of imprudence, professional barons and their slaves, in an attempt to defend their territory, are making haste to scream to the four winds that cold fusion does not exist and is only a fraud. At this point, I am beginning to doubt—in this affair which is becoming more and more obscene—that all the thermofusionists are acting in good faith. . ." Though I had been aware that an unnamed Italian scientist had allegedly

About the Scientist/Inventor

Signor Renzo Boscoli was born in Ferrara, Italy in 1920. He graduated with honors in classic studies from the Liceo Classico in Ferrara in 1939. World War II interrupted his math and physics curriculum at the University of Ferrara, but he continued in the same fields as a member of the "Genio Marconisti" in the Italian military.

The conditions of immediate post-war Italy, and the requirements of family (wife, Giovanna, and two small children), mandated that he be gainfully employed rather than complete his university studies. These efforts included positions as Director of Laboratory Research, until 1980, at such multinational conglomerates as S.I.D.E.S in Vicenza, Italy and RHEEM-SAFIM and FOSFACOL in Milano, Italy.

During these "rebuilding" years, his much-acclaimed work drew laboratory directorship offers from Dow-Corning in the United States and SLLS in France, both of which Signor Boscoli, due to familial circumstances, was forced to decline.

Among his achievements during this post-bellum period were the authorship of twelve pioneering patents on various siliconization processes while at S.I.D.E.S, along with a number of "firsts" in industrial vacuum production/maintenance and zinc coating and phosphatizing processes while at RHEEM-SAFIM and FOSFACOL. In addition, in 1964, Signor Boscoli won a national science competition in chemistry (sponsored by EURATOM) and was intimately involved with early industry-sponsored laser research.

Over the years, his accomplishments led to invited guest participation at numerous international scientific meetings in Switzerland, France, and England. Less well-known is Signor Boscoli's stunning original conceptualization, in April 1948, of the "black box" flight recording/rescue system, which he offered in June 1948 to the United States Embassy in Rome. His concept was subsequently adopted in the United States and other countries as well.

Since 1980, Signor Boscoli has been a scientific consultant to various industrial conglomerates. Presently, his four decade private investigation of nuclear fusion has led to the formulation of an exciting and extremely promising research effort under the sponsorship of Paradigm Energy Corporation. The company's new location is: Bow Technology Center, 3 Robinson Road, Building A-4, Bow, New Hampshire 03304, U.S.A. Website: www.ParadigmEnergy.com Phone: 603-229-3264 Fax: 603-229-3263 made a spectacular breakthrough, no one at *Infinite Energy* had known any details of this research until recent months.

The marvelous news for the cold fusion field is this: In mid-October 1999, an international Patent Cooperation Treaty (PCT)/European Patent Office application will issue for Renzo Boscoli's cold fusion process. The patent is expected to issue soon thereafter. The inventor prefers to call the process "warm fusion," because it routinely generates temperatures exceeding 1,000°C—but let's face it, it's "cold fusion" by "hot fusion" standards. We have been told this by the CEO of Paradigm Energy Corporation—the Florida-based U.S. company which will be developing the technologies that follow from Boscoli's discoveries. Furthermore, the published PCT/EPO application is said to provide all descriptions necessary for a dedicated laboratory or laboratories to replicate the process quickly. If the technology lives up to the claims being made for it, it could be—*by far*— among the most potent of the low-energy nuclear reaction processes heretofore disclosed. Proper and rapid replication of the experiment by other qualified groups could lead to a relatively quick demise of the debate over cold fusion. It could be the proverbial "knock-out blow" for which we have all hoped. All this hinges on the PCT/EPO application disclosure in October being fully adequate for replications to occur.

The Process

Infinite Energy has *not* been shown the details of the experiments, which have been held in utmost secrecy for these many years. This Editor pleaded to Mr. Boscoli, via his representative, to have the experimental details published along with the associated theoretical discussions that appear in this issue. The latter do indeed give a fairly good indication of what this high-temperature solid state process could be like. However, it was seventy-nine year old Mr. Boscoli's wishes that these wait for the actual issuance of the PCT patent application disclosure in October 1999—*Infinite Energy's* website will provide details of this PCT application as soon as we receive them. Boscoli, having been disappointed by what happened to his 1948 conception of the "black-box" flight recorder technology, wishes to be on solid legal ground this time. Also, he appears to be working to perfect the process in the following sense: It is already 100% reproducible, however, the desired heat production— as opposed to certain nuclear emanations— cannot, yet, be counted to occur in each instance. That is, in every case one or more irrefutable effects occur, but it is the critical thermal releases that are in the process of being stabilized to Boscoli's demanding standards. Once that occurs, and this may be only months away, he will go totally public and perhaps come to the United States to be at the forefront of Paradigm Energy Corporation's U.S.-based research.

The Boscoli process is definitely a high-temperature, solid-state reaction. It has been characterized by these limited descriptors:

1) A reaction heat with temperatures exceeding 1,000°C within *seconds* of initiation.
2) Emission of extremely short-wave electromagnetic radiation.
3) Gamma radiation emission.
4) Neutrons at 100 times background and beyond.
5) Power generation at, and exceeding, 1,000% over-unity.

However, much more can be inferred and understood from the Boscoli papers and discussions with our informed source, who is among the handful of human beings who have actually participated in the experiments for the better part of the last decade. It appears that the process has already been made self-sustaining—continuing perhaps for days or longer, provided the requisite nuclear fuel is continuously provided.

After examining the text of his 1984 and 1992 papers, it appears that he is suggesting a solid-state process in which lithium deuteride and other compounds are initially bombarded by gamma rays, neutrons, or deuterons yielding significant thermal energy and ^{4}He in a possibly self-sustaining reaction. He writes, "Theoretically, once the deuterium photodisintegration reaction is primed by the gamma rays, the reaction itself should be self-sustaining. In practice, however, once the fusion process is on-going, it might be necessary, in order to compensate for the inevitable losses, to continue to furnish nuclear fuel, in the form of adequate doses of neutrons and gamma rays (both controlled with opportune moderators and/or screens)."

In his 1992 paper, Boscoli refers to an obscure experiment done by Enrico Fermi and others—the bombardment of heavy water ice by a 200 KeV deuteron beam, yielding, he says, an indisputable cold fusion reaction—albeit intended by Fermi not as cold fusion but as a mere neutron generator! There are hints that Boscoli uses this method as an inspiration for part of his process. It must be said, however, that we do not really know what Boscoli's present method is. We'll simply have to wait for the "October Surprise"—the PCT application being published. Let us hope that it is a pleasant surprise, indeed. We suspect it *will* be. Meanwhile, we are free to ponder other implications.

[**Late Breaking News:** With the encouragement and advice of Cold Fusion Technology, Inc., Paradigm Energy Corporation has acquired a U.S.-based corporate office and research facility (10,000 sq. ft.) at the Bow Technology Center, 3 Robinson Road, Building A-4, Bow, New Hampshire 03304, U.S.A.]

Astrophysical Implications

Should we doubt what astrophysicists have told us about our own Sun—that a core temperature of some 15.6 million degrees Kelvin leads to the net fusion of hydrogen to helium via several thermonuclear fusion processes? Boscoli says that the basic process is actually the *gravitational* fusion of protons and electrons (and anti-neutrinos) to neutrons (see his Equation 4). This is the beginning of a series of endoenergetic (energy-consuming) and exoenergetic (energy-producing) reactions that lead to a *net* unit solar energy process of hydrogen to helium conversion:

$$5p + 3e^- \rightarrow {}^4He + n \quad (+25.939\ MeV)$$

Whereas the Hans Bethe process he describes as *net:*

$$4p \rightarrow {}^4He + 2e^+ \quad (+24.678\ MeV)$$

Boscoli believes that other heavier elements, such as oxygen, are also created in the solar interior—rather than exclusively in supernovae, as is commonly believed. However, he does not go into this projection of the heavier element creation chain in the current articles. In a future presentation in *Infinite Energy* he may continue his thoughts on this cosmogony. He points to current astrophysical mysteries such as the observation of water molecules in sunspots.[5] It may be, he has told his colleagues, that the much disputed icy "small comets" of space plasma physicist Dr. Louis Frank (see *IE* No. 13/14, p. 4) originate inside the Sun, rather than from without. Dr. Frank believes that thousands of snowy comets between twenty and forty tons each strike the Earth's atmosphere each day.[6]

There are many well-documented solar mysteries that Boscoli's solar model *may* be able to solve, though it is not clear yet exactly how.[7,8,9] Bear in mind that we do not need his *entire* solar model to be correct— or *any* of it—for his lab experiments to be valid. After all, by his own admission, he has not quantified his solar model beyond delineating the basic reactions. Here are some solar mysteries and astronomical anomalies that the Boscoli theory might conceivably address:

• **Missing solar neutrinos:** Experiments over the past thirty years confirm that only one-third to one-half of the expected electron neutrinos from predicted solar fusion reactions are picked up by terrestrial detectors. This has led to a crisis, because it is deemed "impossible" to alter existing thermonuclear fusion models of the Sun to explain the missing neutrinos. Consequently, theorists are now working to modify the most fundamental "established" physical laws of the "Standard Model" of particle physics in an effort to patch things up. They are desperately trying to show that the neutrinos really are generated at the expected rates, but that some of them transform in transit to Earth and are thus not detected.[1,10] Perhaps the astrophysicists really should consider that the internal fusion model of the Sun could be wrong, instead of trying to save it by rewriting particle models. Occam's Razor, right?

If there is one glaring deficiency of current solar models, it is their inability to explain the missing neutrinos, yet astrophysicist John Bahcall of Princeton—perhaps the leading expert on the missing neutrino problem—acts as though the standard solar fusion reactions are well-established. That seems something like an article of faith on his part, although he defends it well:[1] "There are now three solar neutrino problems, and it does not appear possible to reconcile the four operating experiments with any modification of stellar physics. . . Many physicists are unfamiliar with stellar physics. They do not feel comfortable judging the plausibility of different solar models, even fanciful ones in which the solar model contains, for example, a central black hole or a non-Maxwellian energy distribution for the nuclei. Some physicists are willing to consider solar models that nearly all astrophysicists would dismiss as unworthy of discussion."

With nearly 100% certainty, Prof. Bahcall is likely to find the Boscoli model "unworthy of discussion," just as he has presumably found other cold fusion results performed in terrestrial laboratories "unworthy of discussion." He prefers to invent new physics "beyond the standard electroweak model."

• **Anti-correlation of sunspots and neutrino flux:** There is an apparent anti-correlation of sunspot activity and neutrino flux. The reason for this is completely unknown.

• **The hot corona:**[11,7] The temperature of the Sun's extended corona is a million degrees K, while the photosphere is only 5,800°K. This profound problem of how a relatively cool surface can give rise to an extremely hot envelope has been completely resistant to solution for the last half century of consideration. Additionally, one velocity component of the solar wind—it is about twice what it should be—has also defied explanation.

• **Differential rotation:** The surface of the Sun rotates faster at the equator than at higher latitudes, a still unexplained fact that has been known for hundreds of years. Moreover, helioseismology has confirmed that this differential rotation persists to deep layers within the Sun—very much against predictions. Deeper still, the rotation trend reverses and does other crazy things.

• **The solar dynamo and magnetic field:**[12] Something as fundamental as the solar magnetic field and its generation is basically unexplained. This phenomenon, in turn, is linked to the sunspot cycle, which in turn is apparently linked strongly to global climate change on Earth.

• **Helioseismology results:**[13,14,15] These spectacular new land-based and space-based analytical tools provide checks on Solar density profiles and the distribution of hydrogen vs. helium within the Sun. There is a huge body of new information coming from these sources, often leading to apparent contradictions among the various aspects of solar structure.

The Immediate Future

Reading the literature on solar mysteries, one is consistently impressed that the solar physicists can look upon their conventional models with straight faces. Clearly, all is not well with these models, one way or the other, whether or not Signor Renzo Boscoli is correct. There really could be "something new in the Sun." If a major revision of solar physics came about which incorporated low-energy nuclear reactions, a host of other astrophysical phenomena might need to be scrutinized. It has been suggested that planets themselves have low-energy nuclear reactions going on inside (see article by Bockris and Mallove in this issue, pp. 29-33), not only the giant outer planets of the Solar System (Jupiter, Saturn, Uranus, and Neptune), but also the terrestrial planets such as Earth (see also the letter by Prof. Steven E. Jones, p. 8).

Returning to earth, it is apparent that in the coming months a wild ride may be in store for us as the critical details of Renzo Boscoli's laboratory achievements become known. If this unfolds in a good way, the people of the world may well have a very short path to clean, infinitely abundant energy. Remember, the fusion energy of heavy hydrogen alone in one cubic kilometer of ocean is the energy equivalent of burning all the known oil reserves on Earth. And, the world's oceans are on the order of a billion cubic kilometers in total volume.

It must also be said that the Boscoli process could transform spaceflight, which has so long been hampered by lack of a compact, nuclear energy source. The process is one that is not only compact, but without the massive shielding problems required of fission or "hot" fusion rocket proposals. It could be much more than a new beginning on the road to the stars. Ad astra— indeed!

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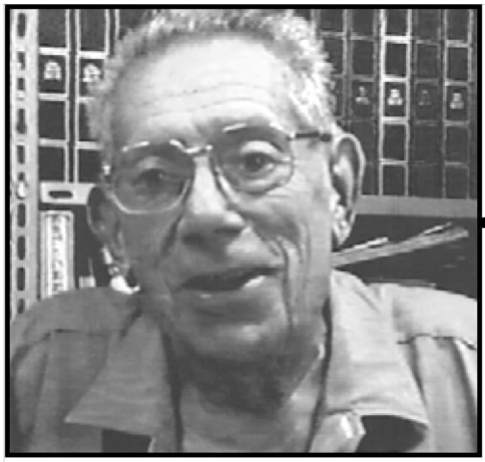

NOTE ON "THERMO" NUCLEAR FUSION

Renzo Boscoli

The universally acknowledged nuclear reactions for the Sun and for the hydrogen bomb are all based on an erroneous transposition of cause with effect. A thermal primer for controlled nuclear fusion would therefore be extremely improbable.

Premise

The following note on *"Thermo" Nuclear Fusion* doesn't pretend to be a theoretical work developed entirely within the scientific method. It is the elucidation of an idea of mine, a discourse made with good intentions that emanate from the uneasiness that I feel upon having seen, for too many years and with increasing frequency, an incredible waste of equipment, time, and intelligence in research—that on controlled nuclear fusion—which I believe to be difficult to obtain via purely thermal means.

As I set out to construct the more exacting components of equipment with which I plan to advance my modest experimental research program on fusion itself, I ask only—though with full respect for the current theories which I do not embrace —that my ideas be heard. I ask neither help nor financing at a time when not to do so seems sheer folly. My only hope is that one of the *insiders*, in examining my ideas, would be inspired to attempt to verify them and—on the long and difficult road toward controlled fusion—also to test the alternative concept that I propose. *June, 1984*

"Thermo" Nuclear Fusion

It is obvious at this time that we know of only two ways to obtain energy from the atom—*fission* and *fusion*. In the case of fission we can say with certainty that we know the mechanism, insofar as it is a phenomenon born and studied in the laboratory, tested in nuclear reactors, and carried to its extreme tragic consequences at Hiroshima and Nagasaki. Concerning fusion, we cannot say that we have in hand equal measures of such cognitive and probative elements.

I know that my assertion will cause those who *know* that they know everything about fusion to grimace when they happen to read what I have to say. But I am equally convinced that those who honestly follow my line of reasoning will find it difficult to escape unscathed with all of their consolidated "certainties" intact.

It is worthwhile to remember how we arrived at our present knowledge of fusion. Omitting the old theories on the origin of solar heat, and without retreating too far into the past, we arrive at A.S. Eddington's modern mathematical theory, developed over sixty years ago, on the internal composition of the Sun and other stars like our own.

The genial Eddington, commencing with given data on the mass of the Sun, its density, and the quantity of heat radiated per unit time, had to arbitrarily assume that the laws of perfect gases were applicable to the Sun, subordinating his working hypothesis to another equally arbitrary hypothesis which maintained that the Sun is composed of a continuous mass without variation—especially qualitatively—in which radiating pressure and gravity are in perfect equilibrium.

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The resulting solar model is *obviously* an enormous ball of gas composed primarily of hydrogen, whose temperature *must rise* gradually, from about 6,000° Kelvin (K) at the external surface (the photosphere), to about 14 to 15 million K in the center. On an equal basis the density also increases—according to the most recent estimates—from values of 10^{-7} g/cm^3 in the photosphere to 134 g/cm^3 in the center. Analogously for pressure, descending into the interior of the Sun from the extremely low values of the photosphere, there is in the center, according to Eddington, a pressure of 1.3 x 10^9 atmospheres. In order to confirm the construction of such a powerful mathematical edifice (that should faithfully mirror physical reality, which, unfortunately, is experimentally unknowable) we arrive, after little more than twenty years, at H.A. Bethe's theoretical confirmation with its carbon-nitrogen cycle, which deservedly becomes a counterbalance to the proton-proton chain. In the end, like the grand finale of a great fireworks show, we witness the sudden explosion of the hydrogen bomb. The Sun was thus "served on a platter" to mathematicians, physicists, astrophysicists, and the military, who together with the politicians, finally breathed a sigh of relief: the *superbomb*—whose construction they wanted and financed (with tax money from the common man)—really worked . . .

But in returning to the "official" model of the Sun, I must reveal that neither Eddington, nor all the others who followed and accepted his theories, ever asked themselves if *their Sun*, a Sun that today is perceived in the same way, could exist—or worse—if a Sun thus constructed could ever be formed. In effect, given the thoughts of those who "know" how the stars are formed, it seems as if the force of gravity is so privileged that one could permit himself to ignore many other of the more noted and sacrosanct laws of Physics. If it's true, in fact, that when a gas is compressed, it heats up, it is equally true that when this gas heats up, it expands . . . It is therefore difficult to understand how gravity could, in a free space not limited therefore by barriers of any kind, continually amass a gas, protostellar in nature, contemporaneously permitting its temperature to continually rise without its automatically and instantaneously

I have decided—after more than twenty years of theoretical research on nuclear fusion—to adventure into the "rough seas" of experimental research on the same subject. I know that I can never, unless by some unthinkable miracle, arrive only by my own effort at controlling fusion. It would be enough for me, moreover, to reap, via my research, even a tiny indication that would confirm the truth of my working hypothesis.

reaching a thermo-gravitational equilibrium that would impede all further contraction of the mass of that same gas. This is one of many small or great mysteries that—too often—"official science" seems to prefer to resolve too lightly.

In the 1930s, the French physicist Georges Ranque discovered that in introducing compressed air into a tube—perpendicularly and tangentially, for a turbine effect—on one side of an internal diaphragm containing a small central hole, the air itself, upon exiting the two extremities of the tube, would be of two diverse temperatures: the air nearest the axis of the tube was cooler than when it entered, and that furthest from the axis of the tube was warmer. In a well-constructed Ranque tube, the thermal difference could reach and surpass 100°C.

This phenomenon is a *disconcerting* phenomenon: disconcerting to accept and disconcerting to explain, in that it seems to contradict the laws of thermodynamics. In order to abstain from the debate involving those who dispute this phenomenon with contrasting explanations, I will not try to give one of my own, which would be of little use to the purpose of my note. I need only to affirm, regarding the Ranque phenomenon—which was studied and taken up later by the German Rudolph Hilsch—that from the cryogenic point of view only, no one—neither astrophysicist, nor astronomer, nor physicist—has ever grasped the importance that this phenomenon could assume were it to be considered in an ambiance less restricting than that of a simple metal tube several centimeters in diameter.

And yet, Ranque's phenomenon (which was almost totally forgotten, especially by the new "recruits" of physics) would merit, in my opinion, promotion to the level of *Principle*, the proclamation of which could be: *whenever any mass of gas, whatever its initial density and temperature, is caused to rotate axially, via exogenous and/or endogenous effects, it will gradually cool in the area near the axis of rotation and will warm in the external areas.*

Indisputably, then, due to the effect of the rotation of the mass of gas, a dynamic separation or extraction of high energy kinetic molecules from lower energy molecules is obtained (which remains and becomes accentuated for the duration of the rotation). If this principle were introduced into areas diverse from those of purely cryogenic technology (in which, to tell the truth, it has not yet experienced the success it merits), other scientific problems could be resolved. Alternatively, other problems thought to be definitively resolved would have to be reconsidered from their inception.

In meteorology, for example, there is the phenomenon of hailstone formation, about which many hypotheses have been advanced, none of which explains it satisfactorily. Notwithstanding the lack of a thorough investigation on the origin of the impressive electrostatic manifestations that accompany the state-transformations of that marvelous "machine" that is water (experiences of H.B. de Sausseure, A. Volta, and W.G. Armstrong), it would be enough to consider what happens between two strata of air, each at a slightly different altitude, moving across each other, propelled by wind cross-currents: in the intermediate friction zone, between the two layers, true "wave trains" of horizontal vortices form and seem to chase one another. The central area of each vortex generates, via the Ranque Effect, cooling sufficient to freeze the little drops of water present. These "ice seeds," continuing to spin, will increase in volume a little at a time, growing at the expense of the surrounding frozen mist, *just as does melted sugar candy in a rotating basin;* the vortices continue in this manner until their weight, having become greater than the sustaining force of the combined action of the wind and the rotation (Magnus Effect: H.G. Magnus, 1802-1870), causes them to fall to the ground. My hypothesis regarding this phenomenon, to me, seems sufficient to explain the concentrically layered spheroidal growth of hailstones.

In this note on *"Thermo" Nuclear Fusion*, I have digressed to treat hailstone formation, only because, patient reader, in extrapolating from the Ranque Effect, it seemed impertinent, when dealing with a hypothetical, to abruptly traverse from a *one-inch* metal tube to the immense cosmic spaces. But even though the journey from a Ranque tube to an atmospheric "turbine" is infinitely smaller than the successive voyage necessary to arrive at the dimensions of a cosmic cloud, to me a meditative pause in a *meteorological parking lot* does not seem irrelevant prior to adventuring into the vastness of the untrustworthy cosmophysics speculation about the space that surrounds it.

Now we have a cosmic mass of gas (that could, although not necessarily, contain more or less uniformly distributed dust), in a condition of isotropic temperature, and in relatively tranquil rotation. The origin of this *mass* of gas and dust is of secondary importance. It could have gradually accumulated during the course of billions of years; it could have resulted from *droplet* fractionation of a larger mass of gas; or, it could have come from a violent and more recent supernova explosion, etc. If, at a certain moment, this mass of gas, for one or more concurrent reasons, would begin to rotate upon itself, along an ideal axis, it would necessarily assume a progressively flatter ellipsoidal form as its rotation velocity increased. And, certainly here too, the Ranque Effect would begin to be exerted, therefore producing a cooling *at the center* and a heating at the periphery of the ellipsoid. Central cooling is the *only possible mode* that allows gravity to attract other gaseous material toward the central area without the obstacle of an increase in systemic kinetic energy, which would counteract the action of gravity. Therefore, due to a constant Ranque Effect, I see no reason why the center would not continue to cool toward absolute zero, also being favorably influenced by Johannes Kepler's Third Law, because during condensation the molecules would travel on progressively shorter radial *orbits*, orbiting faster and faster and thereby augmenting the Ranque Effect itself.

Even though this idea of mine is at least thirty years old, I have never professed it publicly: not so much for fear of peer bias against me, as out of certainty that none of the *insiders* would have ever taken it seriously. But now that the existence of B.J. Bok's extremely cold globules (ca. 10°K) can no longer be doubted, and now that, in addition to this, the Arecibo (Puerto Rico) radio-telescope has recently created a certain amount of confusion among astronomers and astrophysicists with the unequivocal discovery of an enormous *cold hydrogen cloud* (ca. -200°C) that is *rotating on its own axis* at a velocity of 300 km/s, any hesitation about this matter on my part would be unpardonable.

Therefore, returning again to our cloud of hydrogen and dust that is rotating on an axis and contemporaneously cooling, we must admit that simultaneously its density will continue to increase in the zones nearest the center core and, once the continuously decreasing temperature reaches the critical point (33°K), this same gas will begin to liquefy, thereby further reducing itself in volume. This process, it is perceived, will incrementally attract other external material and thus, in the center of this cosmic cloud, a cold, compact, spheroidal core will be formed. At this point, I defer to any astrophysicists who might wish (should any exist) to mathematically order this idea of mine. For now, it's enough that my conscience is clear when I envision the growth of this solar core: becoming increasingly colder, first liquefying and then actually evolving into a solid in its interior, when, due to the increase in internal pressure (itself due to the weight of the supervening strata), the first plausible and possible nuclear reaction occurs. Not, however, that nuclear reaction which forms the basis of the proton-proton chain, which I contest with my own plain common sense. In fact, to transform two protons into a nucleus of deuterium, we must transform one of the two protons into a neutron. And in what way? "Quite simply," state the physicists, who proceed to write:

$$p \rightarrow n + e^{+} + \nu \qquad (1)$$

thereby disturbing antimatter in order to confer a shadow of truth on a nuclear reaction that should be left to the realm of daydreams.

The first (and only) nuclear reaction that, instead, I maintain possible at a temperature near absolute zero at the interior of the solar core is that which can occur at the expense of neutral hydrogen atoms, by *gravitational collapse.* But protons and electrons of neutral hydrogen atoms are not enough to yield neutrons. Neutrinos are also needed: and, there is such an abundance of neutrinos throughout the universe that they will always be available.

We know the process of neutron decay quite well; usually we denote it:

$$n \rightarrow p + e^{-} + \bar{\nu} \ (+\,0.783 \text{ MeV}) \qquad (2)$$

A *free* neutron, therefore, at the end of its life disintegrates, emitting a proton, an electron, an antineutrino, and 0.783 MeV of energy. Physicists, however, when they invert this reaction, imagine (in a fashion that to me doesn't seem correct) bombarding protons with antineutrinos, and write:

$$\bar{\nu} + p \rightarrow n + e^{+} \qquad (3)$$

But this reaction in fact is not the inverse of (2), but simply another. . . reaction! Substantially, it is nothing other than (1).

The true inverse of (2) is instead this:

$$p + e^{-} + \bar{\nu} \rightarrow n \quad (-0.783 \text{ MeV}) \qquad (4)$$

which is an endoenergetic reaction and which, simply translated, says that *in the gravitational collapse of the proton and of the electron, an antineutrino is captured, a neutron is produced, and a quantity of energy equal to 0.783 MeV is absorbed.*

I am sure that the majority of physicists will not like this reaction—either because it can appear too simplistic, or because it poses many other interrogatives, such as that of *synthesis* of the neutron, which is perhaps wrongly considered an elementary particle. In fact, concerning *elementary* particles, the focus today is almost exclusively on the names of the particles, since from the humblest school teacher to the most celebrated of physicists, we hear that matter is essentially composed of three particles: protons, electrons, and neutrons. This assertion is acceptable up to a certain point, at which to me it seems more correct to substitute the term *neutrinos* for *neutrons*. It is true, in fact, that neutrons, after their gravitational synthesis, become *stable* only at that moment when they are set in a nucleus together with protons, and in this condition they act like the other particles; but on the other hand, it is true that *free* neutrons, if not captured, decay very quickly. Nevertheless, the resolution of this trouble I voluntarily leave to physicists, not only because, concerning such matters, I have much more revolutionary ideas than could be included in this writing—and therefore even less acceptable to "official science"—but above all because I am interested only in emphasizing the fact, apparently unexplainable, that the neutron weighs more than the sum of the weights of the proton and the electron. Where does this excess of mass come from? Not from the neutrino, whose mass, even admitting that it has a mass, seems truly negligible. And therefore, if we want to derive this excess mass from a transformation of energy into matter, the problem becomes focused on the nature of the energy absorbed. And since we find ourselves confronted with the phenomenon at hand, that of gravitational collapse, which seems to happen practically without concurrent electromagnetic radiation, the choice falls necessarily on *gravity*, no longer considered simply as a force, but as energy. With this in mind, then, it would be easier to perceive the concept of gravitational wave, while the *graviton* could be placed more acceptably among the other elementary particles.

Up to now, I have spoken of the *solar core* rather than the *solar nucleus*. I must point out that I prefer, in this note, to use the first phrase, reserving the term *nucleus* only for the central part of the atom, thereby eliminating confusion and ambiguity associated with the rapid repetition of the same word with different meanings.

Proceeding with the summary description of the solar model that I propose, regarding the distribution of rarified and ionized gaseous material present in the space between the core and the photosphere, I believe that valid motives exist for hypothesizing the existence of stratification into distinct and confined concentric zones near the photosphere, due to the radiation pressure of the core. These zones, which I call "thermal" zones, are, in my opinion, temporally, concentrically differentiated in successive steps—therefore after the core begins its nuclear activity (see Figure 1, pg. 18). These zones are sites of secondary thermal phenomena caused by nuclear reactions successive to neutron synthesis. It goes without saying that between these thermal zones (the last of which is, as we shall see, the photosphere) and the core, there must exist a space practically *void of material*, occupied almost exclusively by the intense flux of neutrons and gamma radiation emanating from the core itself.

To establish these concepts, without dealing with the details of the calculations, I will say that the temperature of the thermal zone immediately beneath the photosphere must be in the range of 4,000 to 4,500°K. It's difficult, if not impossible, to calculate the number, rotational frequencies, and temperatures of the remaining thermal zones. Starting from the innermost zone, therefore, each of the thermal zones must protect itself from the heat of the successive zone, in part due to the Ranque Effect, but above all due to the intense, incessant radiation flux that continues—as the solar wind—from the core of the sun toward the surrounding cosmic space. Regarding the *core*, my calculations indicate that its median diameter must be between 250,000 and 300,000 km, while its form would be that of a rotating ellipsoid with a slight flattening (enough to keep the closest planets within a narrow zodiacal band), and with a median rotational periodicity about fourteen times less than that of the equatorial solar photosphere. The total mass of the core, then, would constitute virtually all of the total mass of the Sun; the photosphere and the underlying thermal zones would be insignificant with respect to the total mass. The median density of the solar core, according to calculations, should be about 200 times that of water, while in the collapsed zones, the density could reach and perhaps exceed 10^{12} g/cm^3.

And now, before examining the Sun's internal nuclear reactions which I believe follow the synthesis of the neutron, I shall briefly review some of the *advantages* my solar model could bring to the comprehension of certain observed astronomical phenomena.

First, even if it's not the most important, there is the phenomenon of the precessional *anomaly* of Mercury's orbit. For fanatical relativists it is (almost) totally clear. For those, like Robert H. Dicke, who believe instead that Einstein was wrong, it has been necessary to turn to complicated equipment to demonstrate that the Sun, as seen from the outside, is not as round as it seems. But the experimental data has left some shadow of doubt concerning the validity of the demonstration.

If instead, as I think, the mass of the Sun is virtually totally concentrated in its core (concerning which my calculated rotational period agrees, moreover, with that found by Dicke: about 1.8 days), and if this core is *by the nature of matter* a rotational ellipsoid, Mercury's orbit *must* be what it is. It is not necessary, therefore, to revert to the theory of relativity in order to explain a phenomenon that does not exist, because the phenomenon observed is actually one of the proofs at hand of the non-spheroidal nature of the solar core itself.

A solar core, therefore, that isn't spherical but is instead a rotational ellipsoid, must gravitationally drag the equatorial area of the thermal zones and the photosphere with a faster motion with respect to the polar zones. This suggests a simple explanation for the Sun's rotational differential: at the Equator, the photosphere is *closer* to the core than at the poles, and due to the increased gravitational attraction, must rotate more rapidly. On the other hand, this other *anomaly* of solar rotational periodicity that varies in function with the latitude—an anomaly which up to the present has been explained only by incredible and grotesque mental gymnastics—is nothing more than clear proof of the discontinuity of the solar globe. The Sun that we see is solely a slender, spherical halo of rarified gas, maintained at a distance from the central core and kept *illuminated*—like the phosphorous of a fluorescent lamp—by the enormous flow of radiation emanating from the core.

Regarding the solar spots and the causes which make them appear darker (and therefore colder) than the rest of the photosphere—the explanation is, according to my hypothesis, truly banal: despite all of the *inventions* that have been made or will be made in the future regarding their nature, the spots are and will remain only *holes* in the photosphere. If in looking through these holes we see *something darker*, it signifies that this "something" is colder than that which surrounds it. Many other arguments remain to be examined in relation to my model of the Sun, such as the origin of the solar magnetic field—*i.e.*, concerning the *bold* hypothesis that the most central area of the core is in a state of *superconductive metal*, with all of the relative electrical, magnetic, and electromagnetic consequences. But a similar analysis would be beyond the scope of this note. I must, therefore, limit myself to these brief, astronomical assertions upon which I have dared to briefly expound.

At this point, before reviewing the other reactions—those which develop after the *gravitational fusion* of hydrogen to neutrons (which I believe occurs in part in the center of the core and in part in the photosphere and the underlying thermal zones)—it is necessary to make some other preliminary observations. We have seen—in reaction (4)—that during this transformation an excess of mass materializes—a transformation I believe can be ascribed only to gravity, viewed not so much as force, but as energy. Although it is difficult to imagine what happens to a neutral hydrogen atom at the moment in which it implodes into something one hundred thousand times smaller—the so-called neutron—it doesn't seem excessively hazardous to visualize the phenomenon as a sudden emission of gravitational waves instantaneously absorbed in the formation of the proton-electron-neutrino complex. The absorption of these waves, whose corpuscular gravitational nature we cannot disregard, must relativistically balance the value of the neutron's excessive mass.

Moreover, the more interesting aspect of this neutron synthesis concerns the *temperature* of the neutrons in the nascent state. Physicists who study free neutron beams—from a fission reactor, for example—and who liken this neutron flow to that of a gas, by convention use the phrases *neutron gas* and *thermal neutrons;* concerning decreasing associated energy, they speak of *cold, very cold, and ultra-cold neutrons*, even though the parameters which permit this classification are rather uncertain (an exception is made for the ultra-cold neutrons, which are defined with greater precision based on their property of being totally reflected by a solid surface). But while fission neutrons are emitted in the core of a reactor with an initial energy of many millions of electronvolts, with a velocity close to one-tenth that of light, and, while traversing the material that comprises the moderator (for example deuterium oxide), undergoes a progressive slowing due to collisions with the nuclei of the moderator itself (up to the point where the energy of the neutrons becomes the same as that associated with the thermal motion of the nuclei of the moderator), the neutrons produced in the solar core behave in a totally different manner.

Formed, in fact, at practically no energy cost ($< 10^{-7}$ electronvolt) and therefore ultra-cold, solar neutrons not only incrementally acquire energy by a mechanism which we will soon elucidate, but, due to their very low velocity, are in an ideal condition to react with the hydrogen in the immediate vicinity of the zone of collapse. The first of the probable reactions, therefore, I believe to be the following:

$${}^1_1\mathrm{H} + {}^1_0\mathrm{n} \rightarrow {}^2_1\mathrm{D} + \text{gamma rad.} \quad (+\,2.224\ \mathrm{MeV}) \qquad (5)$$

after which could follow the other:

$${}^2_1\mathrm{D} + {}^1_0\mathrm{n} \rightarrow {}^3_1\mathrm{T} + \text{gamma rad.} \quad (+6.239\ \mathrm{MeV}) \qquad (6)$$

But evidently not all the neutrons produced in the core can undergo the two captures indicated above. Many of them, in their wanderings through neutral hydrogen atoms, will reach the end of their existence before having been able to react with any of the surrounding nuclei. In that case, due to (2)—beta decay—they could generate protons, electrons, and neutrinos in peculiar reactive conditions because they are all in the *nascent* state. The protons, in this specific manner, are able to *originate in proximity to* tritium nuclei with which they can immediately react accordingly:

$${}^3_1\mathrm{T} + {}^1_1\mathrm{p} \rightarrow {}^3_2\mathrm{He} + {}^1_0\mathrm{n} \quad (-0.765\ \mathrm{MeV:\ endoenergetic}) \qquad (7)$$

or also, immediately, according to:

$${}^3_1\mathrm{T} + {}^1_1\mathrm{p} \rightarrow {}^4_2\mathrm{He} \quad (+19.824\ \mathrm{MeV:\ exoenergetic}) \qquad (8)$$

Helium-3, in turn, can absorb a neutron to become helium-4, thereby liberating energy according to the reaction:

$${}^3_2\mathrm{He} + {}^1_0\mathrm{n} \rightarrow {}^4_2\mathrm{He} \quad (+20.589\ \mathrm{MeV}) \qquad (9)$$

But other than the nucleosynthetic reactions already seen, that is, *on the way up*, practically all of them characterized by electromagnetic energy emission (gamma rays), we must consider a reaction that I define as being *on the way down*, that is, nuclear disintegration, which represents the photodisintegration of deuterium due to the effect of the radiation liberated in the other reactions:

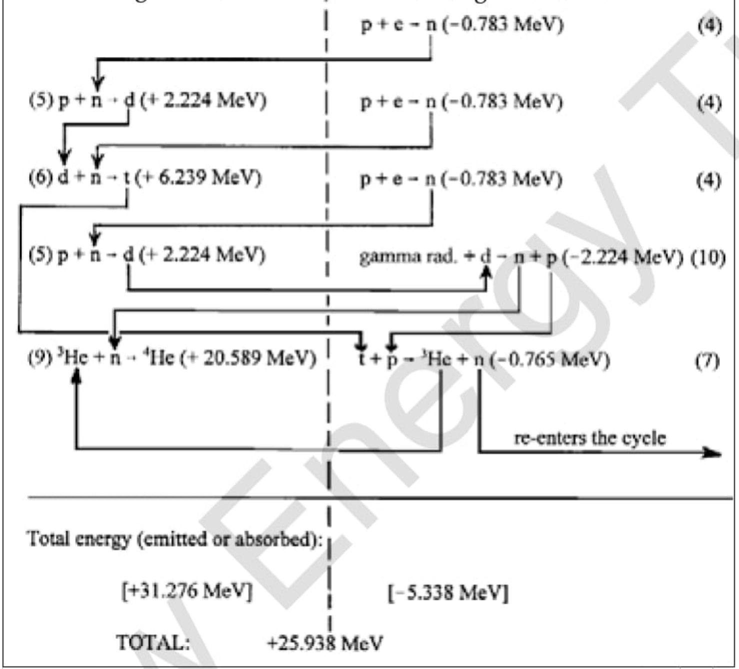
$$\text{gamma rad.} + {}^2_1\mathrm{D} \rightarrow {}^1_0\mathrm{n} + {}^1_1\mathrm{p} \quad (-2.224\ \mathrm{MeV}) \qquad (10)$$

This extremely important reaction is the inverse of (5) and, when it occurs, produces the effect of *virtually* prolonging, in time and space, the lifespan of the free neutron, regenerating it, for example, more externally, in or between the thermal zones, or between these and the photosphere, or in the photosphere itself, or even in the solar corona. The identical effect is obtained with reaction (7).

It's propitious to observe that (7) as well as (10), in each of which a neutron is emitted, are endoenergetic reactions, as is (4) (neutron synthesis), while all the neutron-absorbing reactions are exoenergetic. As far as the velocity and energy of the neutrons are concerned, in correlation to the density of the neutron gas (defined as the median number of neutrons per unit volume), it is evident that—during their existence as such, that is, free neutrons—*they heat up* progressively: either due to the effect of the reactions in which they are involved, or to the *ambient* effect of the continually higher energy zones which they traverse on their path away from the core that produced them, propelled by the solar wind toward the photosphere in which, for the most part, they complete their mission.

Moreover, it's evident that whenever reactions (7) and (10) do not intervene—which, we have seen, virtually prolong neutron life—these same neutrons produced by the core would not have much chance to reach the photosphere unscathed, nor could they arrive with sufficient energy—even if, during a journey equal to the radius of the sun, they were not to collide with any other nucleus. But in addition to this, it must be noted that the electromagnetic energy values reached (via emission/transmission/absorption) are remarkably important in determining the direction and development of the reactions I indicated. In fact, while (5), (6), (7), (8), and (9) are all *fusion* reactions, reaction (10)—which is the inverse of the photodisintegration of deuterium—is, as noted, possible only if and when the acting gamma radiation possesses energy superior to that of the deuteron bond.

To summarize and approximate the energy balance of the solar reactions that I have listed and believe possible, I present the following schematic *sequential cycle*:


Exoenergetic Reactions
Endoenergetic Reactions
p + e → n (−0.783 MeV) (4)
(5) p + n → d (+ 2.224 MeV)
p + e → n (−0.783 MeV) (4)
(6) d + n → t (+ 6.239 MeV)
p + e → n (−0.783 MeV) (4)
(5) p + n → d (+ 2.224 MeV)
gamma rad. + d → n + p (−2.224 MeV) (10)
(9) ³He + n → ⁴He (+ 20.589 MeV)
t + p → ³He + n (−0.765 MeV) (7)
re-enters the cycle
Total energy (emitted or absorbed):
[+31.276 MeV]
[−5.338 MeV]
TOTAL:
+25.938 MeV

Eliminating the common terms before and after the arrows among these nine equations, we obtain the simplified expression:

(A) $5p + 3e^- \rightarrow {}^4He + n$ (+25.939 MeV)

Proceeding analogously for Bethe's cycle, as well as for the p-p chain, we obtain instead:

(B) $4p \rightarrow {}^4He + 2e^+$ (+24.678 MeV)

an expression which completely ignores the electron's important function and which *balances out* only by reverting to the artifice of the positron . . .

In the discourse of the nucleosynthetic reactions which I believe evolve in the Sun's interior—beginning with hydrogen—I stop at helium-4: this limitation seems more than sufficient for the purposes of this note, compounded by the fact that the path to the formation of heavier elements would represent an inopportune intrusion into the quarrelsome field of cosmogony, which I prefer to avoid. Finally, a glance at the sketch of a vertical section of the Sun, based on my working hypothesis, should illuminate what I have up to this point set forth (Figure 1).

Moreover, I should reiterate that in order to simplify my drafting of this note, I have considered the role played by only the most . . . coarse elementary particles, *i.e.*, electrons, protons and neutrons, thereby almost totally neglecting the extremely important function of neutrinos—most significantly in energy transport. In properly treating with neutrinos—or better, the mystery of the Sun's *disappearing neutrinos* — I am now forced to reemphasize and reaffirm my . . . antipathy to Bethe's cycle.

Already by the end of the 1960s in fact—at the conclusion of a series of experiments (conducted by a group of scientists from Brookhaven National Laboratory under the direction of R. Davis) designed to measure the flow of neutrinos emitted by the Sun— the first uncertainties about the validity of theoretical models of the "thermo" nuclear reactions emerged which, according to Bethe's cycle, should take place in the Sun's interior. The quantity of neutrinos *found* was in effect much less than the *calculated* projection. J. Bahcall of the California Institute of Technology was even more explicit about the results of these same experiments when he affirmed that a substantial part of the Sun's energy *cannot derive from the carbon-nitrogen cycle.*

In addition to the trouble of the missing neutrinos, there is the relatively recent discovery (1970s, with official Soviet confirmation on July 1, 1983) that the Sun pulses radially with a prime periodicity of two hours and forty minutes. These results, confirmed in 1976 by the British as well, demonstrate their incompatibility with the most modern theoretical models of the Sun and emphasize the necessity for alternate paths of research in order to better understand our star.

When I devised a *discontinuous* model of the Sun nearly thirty years ago, similar to that which I have just illustrated, in which a central core pulses externally, spherically, and with continuity in a radiating flow against an orbiting photosphere (itself *discontinuous* due to its composition of distinct and independent *bodies*, also known as the famous "spots" observable on the Sun's surface), I immediately thought that the photospheric orbital motion around the core would undergo sphero-radial disturbances due to the effect of the radiation flow from below. In other words a Sun, as I postulated it, would have to periodically *inflate* and *deflate* because every solar spot would have to behave as does a ping-pong ball supported by a vertical jet of air—as, for example, at a carnival target shooting booth. The ping-pong ball, in fact, oscillates up and down, and by regulating the diameter of the air-jet nozzle and the resulting vertical column of air flow, the periodicity of oscillation can be made to be practically constant.

My solar model had become ever more consolidated in my mind. However, because no astronomical observation was able to confirm my hypothesis at that time, my prediction concerning the Sun's radial oscillation constituted a grave handicap for my solar model. Confronted with this dilemma, I chose silence rather than prophecy. But I must admit that this choice was not a happy one . . .

However, now that astronomical observation has manifestly proven me right, I don't deny being convinced that radial pulsation is a general characteristic of all stars—at least those that are similar to our own.

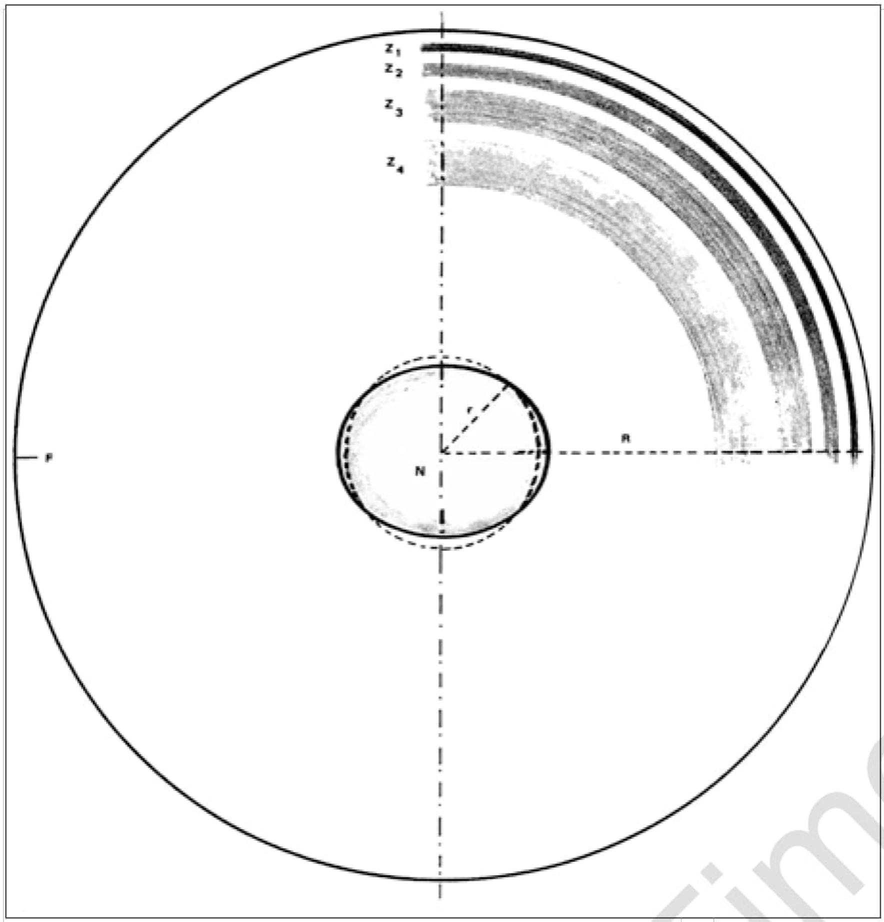


Figure 1. Schematic model of the sun's interior based on the nuclear fusion hypothesis formulated in this note.

The median radius (r) of the SOLAR CORE (N) and the solar radius (R) have, in this sketch, a median ratio of approximately 1:5. The core N is a rotational ellipsoid whose flattening, represented in the sketch, is purely indicative. The total mass of the Sun is contained almost wholly within the core: the mass of the other areas—comprised of the PHOTOSPHERE (F) and the THERMAL ZONES (Z1, Z2, Z3, Z4 . . .)—is negligible. These zones have been designated numerically, since it is not possible to determine with certainty either their number, thickness, consistency, reciprocal distances, temperatures (except for Z1), or their singular rotational periodicities with respect to the corresponding median values attributable to the photosphere.

Concerning the hypothesis regarding concentration of solar mass in the core, the most credible data is that furnished by the calculation of the rotational period for the core itself, which approximates 43 hours. Similarly, an estimate of the core's dimensions places its median diameter between 250,000 and 300,000 km, which corresponds to a median density slightly more than 200 times that of water and a peripheral rotational velocity of about 6 km per second.

Regarding the Sun's internal *temperatures*: these range from near absolute zero at the most internal zone of the core (in which continuous neutron generation occurs due to gravitational collapse of neutral hydrogen—which therefore, proceeding externally, must lead to proportionate temperature increase), to the intermediate values of the thermal zones, and to the highest values in the photosphere (ca. 6,000 K).

Via the solar spots (which, in this Solar Model, are rents in the thin [ca. 1500 km], burning photospheric veil), the first of the thermal zones (Z_1) can be postulated, the temperature of which should not exceed 4,000 - 4,500 K.

Lastly, the imposing flow of both the gamma radiation and the neutrons emitted by the core, as well as the Ranque Effect (which is manifested within the thermal zones themselves), contribute to the thermal protection of one thermal zone from another and from the overlying photosphere— and precludes any relevant heat exchange from the periphery toward the interior of the Sun.

Returning now, after this digression, to complete the discussion of solar neutrinos, I take liberty to note that if my *cycle* were to be considered in the theoretical calculations for solar neutrinos, it would perhaps be possible to "balance the account": it should not be forgotten that in my hypothetical gravitational synthesis of the neutron, for every neutron produced, a neutrino *disappears* . . .

But now it is time to leave the immense energetic richness of the Sun and descend to one of earth's most troubling misfortunes: the *H-bomb.*

Physicists who hope to reach the long sought after goal of controlled "thermo" nuclear fusion desperately cling to this tragic reality, convinced that they are on the right track: that of *heat*. Dazzled by an impossible solar model and rich in the experience acquired with the *H-bomb*, they think that in order to fuse the nuclei of light elements (for example, deuterium and tritium), it is necessary (and sufficient) to surround the mixture of the two isotopes with temperatures of almost one hundred million degrees. According to their calculations, which are actually correct, at these temperatures the kinetic energy of the nuclei should overcome the electrostatic repulsion that tends to keep them separate. And the hypothesized fusion reaction initiated under these conditions would be self-perpetuating, furnishing more energy than that absorbed and/or lost.

In reality, this reasoning may seem correct, in that the primer of the *H-bomb*—actually an A-bomb—instantly generates a tremendous amount of heat. Based on this, physicists postulate:

$$ {}^{2}_{1}\mathrm{D} + {}^{3}_{1}\mathrm{T} \rightarrow {}^{4}_{2}\mathrm{He} + {}^{1}_{0}\mathrm{n} \qquad (+17.6\ \mathrm{MeV}) \qquad (11) $$

thereby liquidating, with this simple reaction, a nuclear mechanism which to me seems a little more complex. In an A-bomb explosion—the same, I repeat, that serves as the H-bomb primer—there is not only heat production, but also a sudden, enormous blast of neutrons and gamma rays. Why, I ask, hasn't the possibility ever been considered that neutrons and gamma rays play a less modest role in the explosion of a so-called "thermo" nuclear device—that neutrons and gamma rays are not simply spectators to the phenomenon, but are instead major protagonists? In other words, it is maintained that fusion in the H-bomb is generated by the heat (*the cause*) of the A-bomb, while neutrons and gamma rays are nothing more than the *effect* of the fusion. And if the inverse is actually true? I support the second hypothesis and re-propose the previously cited reactions for the solar interior, *i.e.*, reactions (10) and (8). The gamma rays of the A-bomb react with deuterium:

$$ \text{gamma rad.} + {}^{2}_{1}\mathrm{D} \rightarrow {}^{1}_{0}\mathrm{n} + {}^{1}_{1}\mathrm{p} \qquad (-2.224\ \mathrm{MeV}) \qquad (10) $$

(photodisintegration of D — an endoenergetic reaction), but the *nascent* protons that are liberated via photodisintegration of deuterium can, in turn, react with tritium to yield helium-4, according to the reaction:

$$ {}^{3}_{1}\mathrm{T} + {}^{1}_{1}\mathrm{H} + {}^{4}_{2}\mathrm{He} + \textbf{gamma rad.} \quad (+19.824\ \mathrm{MeV{:}\ exoenergetic}) \qquad (8) $$

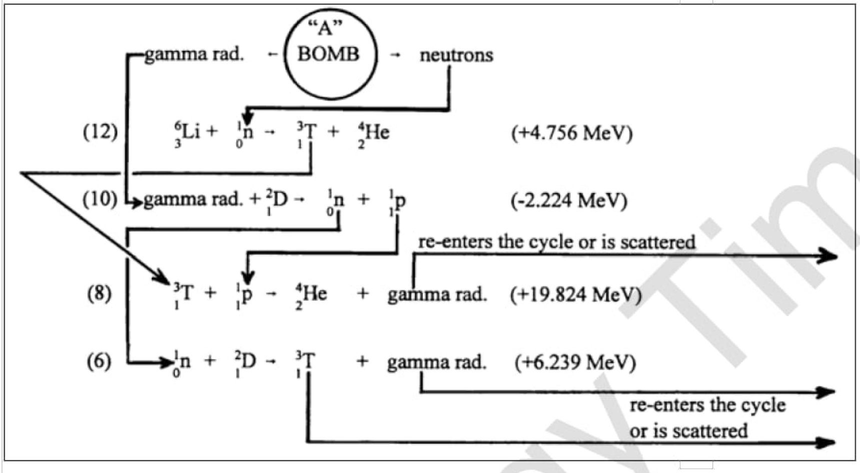
The energy balance of the two reactions (10) and (8) is 17.6 MeV, exactly that of reaction (11) between D and T, which can also be obtained by the *sum* of (10) and (8):

$$
\begin{array}{llcll}
(10) & \cancel{\text{gamma rad.}} + \text{D} & \rightarrow & \text{n} + \cancel{\text{p}} & (-2.224\ \text{MeV}) \\
(8) & \text{T} + \cancel{\text{p}} & \rightarrow & \text{He} + \cancel{\text{gamma rad.}} & (+19.824\ \text{MeV}) \\
\hline
(11) & \text{T} + \text{D} & \rightarrow & \text{He} + \text{n} & (+17.6\ \text{MeV})
\end{array}
$$

But in the H-bomb explosion the above two reactions—the first being primed by gamma rays from the A-bomb explosion, the second producing more gamma radiation than was consumed by the first—are not the only reactions that take place. In fact, the neutrons derived from (10) and from the A-bomb explosion, reacting in turn with deuterium—reaction (6)— regenerate the tritium consumed in (8) and furnish additional gamma radiation to the reaction in progress. In reality then, in place of tritium, the H-bomb physicists prefer—as is noted—to derive nascent tritium from an isotope of lithium (lithium-6), presumably as ^{6}LiD (lithium-6 deuteride), according to the reaction:

$$ {}^{6}_{3}\text{Li} + {}^{1}_{0}\text{n} \rightarrow {}^{3}_{1}\text{T} + {}^{4}_{2}\text{He} \qquad (+4.756\ \text{MeV}) \qquad (12) $$

In summing up, by beginning with the A-bomb explosion, we can propose the following sequence/cycle:


"A" BOMB
gamma rad.
neutrons
(12)
$^{6}_{3}$Li + $^{1}_{0}$n → $^{3}_{1}$T + $^{4}_{2}$He
(+4.756 MeV)
(10)
gamma rad. + $^{2}_{1}$D → $^{1}_{0}$n + $^{1}_{1}$p
(-2.224 MeV)
re-enters the cycle or is scattered
(8)
$^{3}_{1}$T + $^{1}_{1}$p → $^{4}_{2}$He + gamma rad.
(+19.824 MeV)
(6)
$^{1}_{0}$n + $^{2}_{1}$D → $^{3}_{1}$T + gamma rad.
(+6.239 MeV)
re-enters the cycle
or is scattered

Totals:

$$ {}^{6}_{3}\text{Li} + \cancel{{}^{1}_{0}\text{n}} + {}^{2}_{1}\text{D} + \cancel{{}^{3}_{1}\text{T}} + \cancel{{}^{1}_{1}\text{p}} + {}^{1}_{0}\text{n} + {}^{2}_{1}\text{D} \rightarrow \cancel{{}^{3}_{1}\text{T}} + {}^{4}_{2}\text{He} + \cancel{{}^{1}_{0}\text{n}} + \cancel{{}^{1}_{1}\text{p}} + {}^{4}_{2}\text{He} + {}^{3}_{1}\text{T} \quad (+28.595\ \text{MeV}) $$

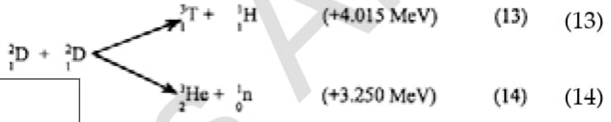
$$ \text{Therefore:} \quad {}^{6}_{3}\text{Li} + 2\,{}^{2}_{1}\text{D} + {}^{1}_{0}\text{n} \rightarrow {}^{3}_{1}\text{T} + 2\,{}^{4}_{2}\text{He} \qquad (+28.595\ \text{MeV}) $$

In reality, however, only a part of the products of reactions (8) and (6) re-enters the cycle, because the heat developed by the A-bomb and magnified by the H-bomb—even though I consider heat to be the *effect* and not the *cause* of the explosive phenomenon of the superbomb—engenders the unmistakable effect of stabilizing and moderating a phenomenon that could truly become paroxysmal, with incalculable consequences. Specifically because of the effect of the heat, the entirety of the *nuclear fuel* mass will be consumed by expanding and dissipating until the whole phenomenon extinguishes itself. It is most fortunate, rather than disastrous, that this occurs, because otherwise, instead of the explosion, a surely catastrophic chain reaction could take place. The possibility of such an event should sober *those who are unapprised* and who so lightly initiate such explosions underground or underwater.

With these premises stated in these terms, and with such apparent desecration on my part of the phenomena of solar and H-bomb fusion, and as the harbinger of a *cold solar core* and of a *revolutionary and heretical* interpretation of phenomena wrongly (I believe) defined *"thermo" nuclear*—I am certain that I don't have much chance of being taken seriously by "insiders;" to raise doubts about the feasibility of controlled *"thermo"* nuclear fusion is perhaps severely prohibited.

When, in 1958, the scientists of the Atomic Energy Research Establishment at Harwell announced that they were the first to produce controlled fusion with their ZETA device in which, in confirming their complete success, neutron emission was authenticated—the whole scientific world was in an uproar: those days were filled with enthusiasm, criticism, approval, dissent, and denial—all in a fantastic turmoil. But then, faced with the facts—the ZETA device did not produce even one atom of helium or tritium—the British had to admit to having blundered.

This almost totally forgotten episode, after twenty-five years of other negative results, does not merely represent the first of great failures in the course of research on controlled fusion; in my view it was also a great opportunity lost. The physicists at Harwell, in fact, proposed to accomplish fusion of deuterium atoms with their Zeta device, according to the following reactions:



$$ {}^{2}_{1}\text{D} + {}^{2}_{1}\text{D} \rightarrow {}^{3}_{1}\text{T} + {}^{1}_{1}\text{H} \qquad (+4.015\ \text{MeV}) \qquad (13) \qquad (13) $$

$$ {}^{2}_{1}\text{D} + {}^{2}_{1}\text{D} \rightarrow {}^{3}_{2}\text{He} + {}^{1}_{0}\text{n} \qquad (+3.250\ \text{MeV}) \qquad (14) \qquad (14) $$

each of which should occur with practically equal probability. But it is evident that the neutrons generated in the ZETA's "ring" could not have originated from either (13)—which does not provide for neutron emission but only for production of tritium and protons, or from (14)—because helium-3 would have had to have been produced along with the neutrons. But if no trace of neutrons was found, the most plausible hypothesis is that a different reaction than that which was desired occurred in the ZETA apparatus. And what reaction would that have been? The deuterium plasma that was to undergo fusion was certainly composed of a mixture of deuterons and free electrons. It is also known that the collision of a stream of electrons—however they are accelerated—with an obstacle induces the emission of X-rays or gamma rays, according to the energy possessed by the electrons at the instant of collision. On the other hand, the differences between X-rays and gamma rays lie only in their correlative wavelengths; additionally, there is no net limit of separation between them because their respective energy spectra are superimposed in the frequency interval between $3 \bullet 10^{19}$ Hz and $3 \bullet 10^{20}$ Hz, or, in the wavelength field between 0.1 and 0.01 Å.

We have previously seen, though, that deuterium's photodisintegration reaction (10) is possible only when the acting gamma radiation energy is greater than deuteron bond energy. And because this bond energy equals 2.224 MeV, the gamma ray energy necessary is:

$$ h\nu_{\text{(MeV)}} > 2.224\ \text{MeV} $$

This inequality is satisfied by a gamma ray frequency equal to $0.5382 \bullet 10^{21}$ Hz, which corresponds to a maximum wavelength (lambda) of $5.574 \bullet 10^{-3}$Å. If now, in an attempt to photodisintegrate deuterium, we should want to utilize an apparatus of the type used to produce X-rays, enhancing its dimensional characteristics correlatively in order to obtain gamma rays with the necessary energy, we would find ourselves in serious engineering difficulty, because—by the Law of Duane and Hunt, which links the wavelength of the radiation produced to the excitatory tension—we would have to apply more than 2,216,000 V to the ends of the tube!

This undertaking would instead be much easier via utilization of a *Betatron:* this accelerator is already commonly used in industry and is commercially available in models capable of furnishing gamma ray beams with a kinetic energy equal to or greater than 30 MeV. But if we compare the design schemes and functioning principles of a Betatron and a ZETA apparatus, we discover some truly surprising analogies. We must admit, though with a discrete dose of imagination, that the ZETA device engenders, from its inception, the qualifications for a possibly anomalous function (with respect to the projections of its inventors)—by producing, with gamma ray emissions of sufficient energy to prime, within the deuterium plasma, the photodisintegration of the deuterium itself. In other words, the ZETA device, in the 1958 experiment, would have partially functioned as a Betatron. To me, this could be one of the causes—if not the sole cause—not so much of the failure of the device as of the . . . unjustified presence of neutrons in the absence of the desired fusion reactions.

I mentioned earlier that the failure of this reactor represents a lost golden opportunity. After twenty-five years, during which time I have quite often returned to the unsolved problems left behind by this experiment, I am convinced more than ever that the physicists at Harwell—and along with them all those who have since been occupied and preoccupied with the experiment's failure—never doubted that they came involuntarily close to their goal. At the cost of noisome repetition, I insist that it was impossible that the experimenters erred in evaluating the nature of the particles formed in their "ring" device: neutrons they were and neutrons they would still be, should the experiment be repeated under the same conditions. But they were perhaps photodisintegration neutrons, according to reaction (10) that I previously indicated as the first step toward fusion—the completion of which would also necessitate the priming of reaction (8) in order to obtain that which is ultimately necessary . . . nascent protons and nascent tritium, which in the ZETA "ring" were non-existent. But even if tritium were present, most probably controlled fusion would not have occurred, due to the plasma's excessively high temperature. This may seem paradoxical, because for decades physicists the world over have been pushing to reach ever higher plasma temperatures. I instead propose to operate not only at low temperature but to quickly siphon off heat as it is produced—analogous to that which must be accomplished in a fission reactor. Wasn't perhaps Enrico Fermi's success, in this regard, due to his providential idea to slow down (which is equivalent to *cooling* down) the neutrons?

Now then, since I know that the most ferocious criticism leveled at me will involve reaction (8)—because both the tritium ion and the proton are positively charged, and therefore forcing them to collide together will require an increase in their respective kinetic energies, as in the case of "phantomatic" reactions (11), (13), and (14) already treated—I am obliged to stipulate the following: it is one circumstance to violently separate the electrons from atoms of hydrogen and/or its isotopes, thus freeing up their positive charge, and then collide one with the other under the pretext that they will fuse; it's quite another circumstance to *generate* tritium [reaction (12)] and protons [reaction (10)] contemporaneously at the same site. In this second situation, to my way of thinking, the so-called *short distance forces* can intervene and actualize fusion reaction (8), due to the fact that the particles which must react together are not separated by great distance and an insurmountable barrier of potential, but are close together in a condition practically equivalent to that of the barrier of potential being non-existent—just as I propose happens in an H-bomb explosion. And concerning the H-bomb—I am in essence restricted to diagramming the function that would occur principally, if not exclusively, according to the above-cited reactions (10) and (8), preceded by (12) and followed by (6). This limitation is obviously due to my sparse knowledge of the engineering details of the explosive device. These details, in fact, are not only kept secret but can also change significantly from manufacturer to manufacturer and from bomb type to bomb type—above all relative to the nuclear fuel utilized. It would therefore be highly inferential on my part to delineate, from the little generic information on the subject that I possess, a more detailed model of the explosive phases themselves.

> *"after more than twenty years of theoretical research on nuclear fusion—to adventure into the "rough seas" of experimental research on the same subject. I know that I can never, unless by some unthinkable miracle, arrive only by my own effort at controlling fusion."*

This being said, and before continuing on to the exposition of what I propose to be a possible alternative path to controlled nuclear fusion, I must make yet another critical observation on what up to the present, in this field, has been sustained and is continuing to be maintained at this moment. It seems to me in fact, among other things, that the question of the high temperature necessary to obtain fusion includes a not insignificant conceptual equivocation—that of the correspondence between the effective temperature of a body that is emitting or absorbing energy, and its *kinetic* temperature. The first instance must in effect be considered *actual*, while the second may also be only *virtual*. When, for example, we say that energy and temperature are coupled in the analogous relationship:

$$1\ \text{eV} = 1.16 \cdot 10^4\ \text{Kelvin}$$

we must not even remotely think that the liberation of energy equal to 1 eV is accompanied, *every time*, by a thermal phenomenon capable of elevating the temperature of the system in which it occurs by more than 10,000°K. If this were the case, in fact, the lighting of a simple household light bulb should cause a veritable thermal pandemonium! In other words, it seems justifiable to dispute to the bitter end the reversibility of this correspondence, which fortifies physicists worldwide in this field with the feeling that they are authorized to proceed with extremely costly programs, which I believe, if based solely on this "thermal" factor, are all destined to fail.

Moreover, even if the hypothesis of "thermo" nuclear fusion were to be accepted as feasible, the goal of 100 million K that is being sought does not represent an end in itself, but only a new departure point. In fact, at that temperature, according to the most recent estimates, the Q ratio—that is, the ratio between the energy produced by the fusion reaction and the energy spent to heat the plasma and maintain the functioning of the apparatus—should become equal to 1. Reaching this first important result should constitute the demonstration of the controllability of a plasma in an apparatus like those recently projected and, therefore, confirmation of the feasibility of "thermo" nuclear fusion itself. But it's inevitable that in order to supersede this value by that amount which is necessary to obtain utilizable energy, it will be necessary to elevate, and not just by a little, the temperature of the plasma in the reactor. If you consider that at the present state of research this Q ratio is only a few hundredths of unity, you cannot help but admire the faith and hope of the "insiders," as well as the charity of those who furnish the means to construct these apparatuses.

But to the great and numerous engineering difficulties still to be overcome are to be added others no less important: as the temperature of the plasma is incrementally pushed higher and higher, the possibility of increasing *pollution* of the plasma, with undesirable induced radioactivity, is lying in wait. This fact will inevitably lead to not indifferent construction complications, and will present not easily solved problems to the physicists and engineers involved; and—the most grave problem—they will realize that fusion energy is not as clean as was hoped . . . And this is not all. Magnetohydrodynamic complications could also develop such that they would disturb and compromise plasma stability itself. This would be equivalent to having to reexamine many of the results so arduously obtained.

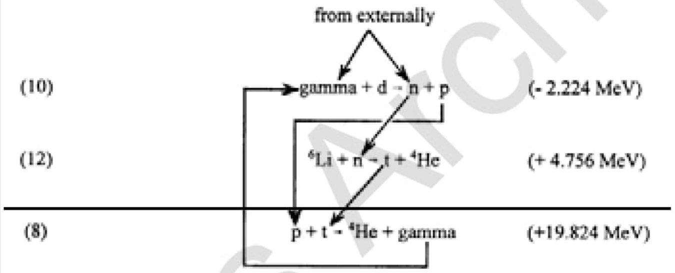
On the other hand, it cannot be established with certainty that all research programs already in progress are proceeding in complete tranquility. As physicists little by little plan and construct their devices, many perplexities will emerge concerning the results which they hope to attain. In fact, not only are tentative objections being raised by some of the "insiders," but even a reflourishing of initiatives has been noted on ideas that seemed to be out of date. For example, along with research alternative to that of the Tokamak—what is emerging as an inertial confinement system based on the use of powerful lasers, instead of magnetic confinement—talk of the *muon* system (also described, improperly, as *meson*, μ) has resurfaced. According to this method—theoretically unexceptionable and of undoubted scientific interest—it should be possible to realize *low temperature* nuclear fusion in which the muon possesses the ability to form mesic atoms: for example, with deuterium atoms in which the muon substitutes the atomic electron. Such an electrically neutral atomic system could therefore approach another deuteron without undergoing repulsion—thus forming a mesic molecular ion composed of two deuterons bound closely together, attributable to the effect of the muon orbiting around both.

The probability of this event should be about the same as that which is verified in a fusion reaction of two deuterons with consequent energy liberation. Moreover, according to projections, the muon could be expelled during the phenomenon and could be recycled in order to create other mesic atoms and to prime other fusion reactions. To contemplate this, however, means taking into account the muon's brief lifespan (two millionths of a second), and of the probability of its being captured by a helium nucleus produced in the fusion reaction. If you then compare the energy that must be spent to *create* a muon with the energy which the muon might furnish as a catalyst of the fusion reaction, you quickly realize that fusing deuterium in order to produce helium by utilizing muons is a very meager affair.

Now, it's unthinkable that "insiders"—even though "dusting off" and re-proposing the muon after thirty years—are not apprised of these facts. I believe you could promulgate one line of reasoning only—that some physicists, in their studies on the muon, have discovered something which encouraged them to proceed with research about which their no-less-illustrious and authoritative colleagues have recently openly expressed negative opinions. But over and above this already disquieting interrogatory, what could be the reason for which scientists of undisputed fame busy themselves with the search for alternate paths to atomic "thermo" fusion? Probably because serious doubt about its scientific feasibility looms on the horizon.

It would be extremely interesting to research this aspect of the situation, citing all of the opinions of those studious researchers who, even via contrasting motives, do not demonstrate to be very convinced of the feasibility of controlled "thermo" nuclear fusion, but this discussion would augment the length of my note too much. And, therefore, in conclusion I will summarize what I have previously set forth to be the elements that characterize my fusion proposal as an alternative to "thermo" nuclear fusion.

For those who have followed me this far, by now it is clear that I suggest operating at reduced temperature and pressure, subjecting the nuclear fuel (which, for example, could be composed of lithium-6 deuteride) to the combined action of neutrons and gamma rays with a wavelength of less than 5.5 • 10^{-3} Å, according to the following outline:



and in summation:

$$d + {}^6Li \rightarrow 2\,{}^4He \quad (+22.356\ \text{MeV})$$

Theoretically, once the deuterium photodisintegration reaction is primed by the gamma rays, the reaction should be self-sustaining. In practice, however, once the fusion process is ongoing, it might be necessary, in order to compensate for the inevitable losses, to continue to furnish nuclear fuel, in the form of adequate doses of neutrons and gamma rays (both controlled with opportune moderators and/or screens). Because, however, you could not exclude, *a priori*, that reactions (10), (12), and (8) could produce a *chain* reaction conceivably of an explosive character, perhaps the best solution would be to invert the process. Therefore, in a situation in which you could *channel* gamma rays and neutrons in a continuous flow, you could pipe in the deuterium, while the lithium-6 could be present in various forms such as dust, liquid, vapor, or actually as an integral part (cladding) in the reactor itself. Other than this, the isotope Li-6 is not strictly indispensable in that you can obtain tritium from the Li-7 isotope according to an endoenergetic reaction which, in contrast to (12), restores the incidental neutron:

$$ {}^7_3Li + {}^1_0n \rightarrow {}^3_1H + {}^4_2He + {}^1_0n \qquad (-2.495\ \text{MeV}) \qquad (15)$$

Now, because lithium-7 and lithium-6 are the two natural isotopes of lithium, with respective ratios of 92.58% (Li-7)/7.42% (Li-6), you can see that it would be opportune to use natural lithium, thereby eliminating the costly preliminary separation of the two isotopes, even though this substitution (keeping in mind the ratio of Li-7/Li-6) imposes an energy sacrifice equal to 6.7 MeV.

From that which I have said up to now, it must be evident that I don't have the slightest intention of proposing my own project for a fusion reactor in which to mix together, as in a "magic" card game, neutrons, gamma rays, deuterium, and lithium. With this note, I intend only to present an idea. This idea I believe to be—perhaps wrongly, and with a discrete dose of presumption—no worse than those up to now put forth by "official science."

As I have already said in the premise of this note, I have decided—after more than twenty years of theoretical *research* on nuclear fusion—to adventure into the "rough seas" of experimental research on the same subject. I know that I can never, unless by some unthinkable miracle, arrive only by my own effort at controlling fusion. It would be enough for me, moreover, to reap, via my research, even a tiny indication that would confirm the truth of my working hypothesis. And because I don't know if and when and how far I can reach, I invite those who have neutron and gamma ray sources on hand to *also* attempt the path that I have just above indicated.

To summarize and conclude my monograph then, the ingredients of the recipe that I have proposed are: deuterium, nascent tritium (via neutron action on lithium), and gamma rays with energy greater than 2.224 MeV (to photodisintegrate the deuterium at the same instant and site as that of the nascent tritium). This is a project that has been easy enough for me to formulate—the realization of which, however, I do not envision as easy. But the possible technical difficulties of the experiments that I propose are certainly not greater than those involved in the Tokamak or laser reactors—with respect to which however, my proposal seems to enjoy the indisputable advantage of low cost in terms of time and money.

In presenting the "insiders" my modest tome on nuclear fusion, I already know what punishment awaits me: from indifference (the most serious and feared) to the terrible but innocuous broadsides with which I am already familiar—*i.e.*, mathematical formulas that demonstrate that my idea is unrealizable, either because it is countercurrent to present solar theories and to fusion, or because in expounding it I am *surely* trapped in gross error . . . I refuse to accept, however, the verbal challenge and thereby commit myself to sterile diatribe with persons who, with difficulty, would make an effort to understand a language different from their own. I will instead defend my "crazy" idea with a weapon that very frequently has demonstrated to be unbeatable—that of obstinacy. The history of Science is studded throughout with episodes regarding stubborn people like myself—who have, however, contributed to scientific progress by utilizing their time to oppose "official science."

For this reason, and with only my strong convictions, I will continue to plan and construct my "infernal" *devices*, in the hope of not only arriving at the results that I wish, but also above all to learn, one day in the not too distant future, that some Friend, possessing those means that my poverty does not allow me, is making progress and is preceding me on that path which I, with humility and unselfishness, have pointed out . . .

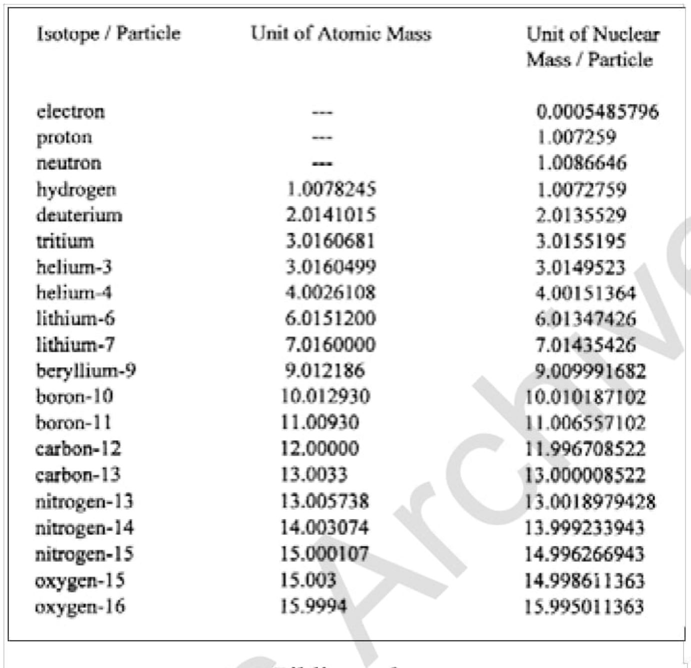
Notice

For the calculation of energy freed or absorbed in the nuclear reactions reported in the text of this note, I have utilized the values of *Unit of Mass* of the particles or of the nuclei that are charted in the following table.

For the greater part, these values have all been taken from the most recent publications on the subject matter. For *Unit of Nuclear Mass*, I have used that which has been adopted since 1961 by the International Union of Pure and Applied Chemistry: that is, one-twelfth of the mass of an atom of ^{12}C, and therefore

$$1\ \text{UM} = {}^{12}\text{C}/12 = 1.660566 \cdot 10^{-24}\text{g} = 931.5016 \pm 0.0026\ \text{MeV}.$$

In various instances I have had to introduce corrections, particularly in the last decimal numbers of these values, in order to equilibrate the data originating from diverse sources. These data, in fact, don't always agree, more often because of inopportune rounding off performed on the first decimal numbers, or more rarely, because of some misprint.



Isotope / Particle	Unit of Atomic Mass	Unit of Nuclear Mass / Particle
electron	---	0.0005485796
proton	---	1.007259
neutron	---	1.0086646
hydrogen	1.0078245	1.0072759
deuterium	2.0141015	2.0135529
tritium	3.0160681	3.0155195
helium-3	3.0160499	3.0149523
helium-4	4.0026108	4.00151364
lithium-6	6.0151200	6.01347426
lithium-7	7.0160000	7.01435426
beryllium-9	9.012186	9.009991682
boron-10	10.012930	10.010187102
boron-11	11.00930	11.006557102
carbon-12	12.00000	11.996708522
carbon-13	13.0033	13.000008522
nitrogen-13	13.005738	13.0018979428
nitrogen-14	14.003074	13.999233943
nitrogen-15	15.000107	14.996266943
oxygen-15	15.003	14.998611363
oxygen-16	15.9994	15.995011363

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October, 1992: UPDATE ON "NOTE ON 'THERMO' NUCLEAR FUSION"

Renzo Boscoli

More than eight years have passed since June of 1984 when, after extended reflection on the matter, I decided to publish my "Note on 'Thermo' Nuclear Fusion." Even though, in so doing, I had foreseen that indifference to my work on the readers' part would be the most serious and fearful punishment that I could receive for having dared to expose a decades-long error of "official science," I continued to hope that some "insider" would one day open his eyes. Up to the present this, unfortunately, has not happened: not one, in fact, of the "barons" of physics has ever deigned, with seriousness and fairness, to formulate any kind of opinion whatsoever—whether of approval or reproach—of the ideas and proposals contained in my "Note on 'Thermo' Nuclear Fusion."

In an effort to change the convictions of those who support the "thermo" nuclear theories of fusion, I have, in a certain sense, found their objections—which I have encountered one after the other—to be justifiable. In fact, I thought that it would be difficult, if not impossible, for those who had cultivated these studies for many years, to overturn inherited and consolidated "certainties" in order to accept the heretical ideas of an unknown who proposed a solar model and a mechanism of the nuclear reactions of the sun and the H-bomb which desecrated official wisdom. However, what I found most perplexing was the "thermofusionists'" systematic refusal to accept a calm confrontation with those, like me, who did not accept their dogma. Newspaper, radio and television interviews, press conferences, seminars and lectures in institutes of higher learning —all came to nought. It seemed as if "official science," with the worst arrogance possible, was hiding behind the shield of its undisputed and unquestionable authority.

The sands of time, meanwhile, indifferent to human events, persevered in their uncontrollable flow. It was thus that we arrive at those two fateful days in March, 1989—the 23rd and 24th—during which the explosive news of Martin Fleischmann and Stanley Pons' electrolytic cold fusion, with Steven Jones' successive polemic addition—spread throughout the whole world. In one stroke, the hue and cry of the thermofusionists raged against the three, accusing them of having offended the dignity of "true science." For awhile, Jones returned to the "shade." Fleischmann and Pons remained at the forefront to blunt the ferocious attacks of the thermofusionists. The scientific world split in two: as usual, there were believers and nonbelievers, those who applauded and those who could only descend into facile bitter irony.

Even in Italy the most heated denigrators of cold fusion are precisely those who cannot completely hide their own fears. Demonstrating the zenith of imprudence, professorial barons and their slaves, in an attempt to defend their territory, are making haste to scream to the four winds that cold fusion does not exist and is only a fraud.

At this point, I am beginning to doubt—in this affair which is becoming more and more obscene—that all the thermofusionists are acting in good faith; instead, among them nest not a few who are "on the take"—who, in order to get ahead, have assured themselves lavish salaries and fabulous "deals," supported by billions of dollars of financing guaranteed up to and beyond the year 2030. Common people, who depend solely on the mass media for their information, are convinced that by that time we'll witness the realization of a fusion reactor capable of furnishing energy at will.

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Demonstrating the zenith of imprudence, professorial barons and their slaves, in an attempt to defend their territory, are making haste to scream to the four winds that cold fusion does not exist and is only a fraud.

But the reality is much more desolate, because by that time, scientists at the maximum will only be able to state whether controlled thermonuclear fusion is feasible or not. If the answer is affirmative, humanity will have to wait until at least 2050—it is thought—in order to witness the historic ignition of this, for now, elusive reactor. If instead, as I believe, the response will be negative, it will be necessary to postpone this joyous event again for several decades, provided that in the meantime the financiers of this insane undertaking will not have opened their eyes and closed their checkbooks.

Meanwhile, those who believe in cold fusion are not twiddling their thumbs. Throughout the entire world, serious research is being carried out without bias of any kind. And finally, the recantation of all previous denials has arrived from the most authoritative Japanese and Russian laboratories: cold fusion is not a mirage but a real phenomenon that is readily reproducible at will! I would not like to be one of those presumptuous Italians or foreigners who, to make a name for themselves (which they did not already have), hastened their own demise with the battle cry of "Crucify!" I don't need to name names: they can be found in all the newspapers. These merchants of false wisdom are now quiet, pretending that they've done nothing; but the disrepute into which they have fallen is not easily canceled or forgotten.

In my 1984 "Note on 'Thermo' Nuclear Fusion" I reported some confirming evidence about that which I was writing. After its publication, other evidence came to light, either from recent acquisition or actually "raised from the dead" of the always fertile cemetery of forgotten or willfully denied facts—by those who keep the keys of "Knowledge."

I find myself constrained to bring to light, for those who have the patience to follow through, these new facts which "official science" has had, against its will, to record—and which transform some of my fortunate intuitions into probative reasoning and "on the mark" forecasting.

I would like to point out that in the factual chronology that follows, each entrance will be consecutively lettered and then followed by two dates: first is the date on which I either produced or learned of the event; the second is the date on which it was chronologically verified.

A) Detonation of the first hydrogen bomb 1952; 1952
B) Rediscovery of the Ranque Effect 1953; 1933 (Georges Ranque)
C) Note on "Thermo" Nuclear Fusion June 1984; June 1984
D) Observation on the birth of a star June 1986; July 1986

After the detonation of the first H-bomb, and after my rediscovery of the Ranque Effect and the publication of my "Note on 'Thermo' Nuclear Fusion," astronomers at the University of Arizona scored a precious point in favor of my cryogenic hypothesis on the genesis of stars and on the internal composition of the Sun (cold solar core). They, in fact, announced the first observation of the birth of a star. This star, denoted IRAS 16293-2422, is an extremely cold, nascent star 520 light years from earth which, though exhibiting a temperature of 196°C below zero, emits twenty times the energy of our Sun.

E) Origin of the Hailstone Phenomenon January 1988; 1850

As cited in the initial pages of my "Note on 'Thermo' Nuclear Fusion," in an effort to find a meteorological verification of the Ranque Effect (as applied to hailstone formation)—I had hypothesized, concerning the origin of the phenomenon, that it might be sufficient to have two strata of air, each at a slightly different altitude, moving across each other, propelled by wind cross currents. "In the intermediate zone . . . between the two layers" I wrote, "true 'wave trains' of horizontal vortices form and seem to chase one another, the central area of each one generating, via the Ranque Effect, cooling sufficient to freeze the little drops of water present. These 'ice seeds,' continuing to spin, will increase in volume a little at a time, growing at the expense of the surrounding frozen mist, just as does melted sugar candy in a rotating basin . . ."

These assertions were only the fruit of my insight, which would have needed objective confirmation. Well, in 1988, that is, four years after the publication of my "Note on 'Thermo' Nuclear Fusion," I discovered in lesson 68 on page 351 of the fourth edition of *Lessons in Physics*, by Carlo Matteucci[1] (Pisa—Pieraccini Printing House, 1850) the following affirmations on hailstone formation:

> "It's certain, then, that beads of hail are suspended for a given time in air (N.D.R.— Magnus Effect): and many recent observations made high in the mountains and also in the middle of thunderstorm clouds establish that the noise produced by the collision, by the banging together of the hailstones, lasts for a while.[2] These observations also seem to demonstrate that two very strong air currents of opposite direction within the same layer or at different altitudes precede hailstone formation."

I believe that these facts speak for themselves!

F) The Nevada Experiment March 1988; March 1986

On March 22, 1988, Italian newspapers published this news report (which I considered to be sensational), reported the day before in the *New York Times*:

> "Two years ago, at the underground nuclear test site in the Nevada desert, a successful nuclear fusion experiment took place."

I would like to point out that this experiment, which was probably performed in March of 1986, followed by about twenty months the arrival of copies of my "Note on 'Thermo' Nuclear Fusion" (which I published in 1984) at the principal American scientific laboratories. In order to carry out the experiment, the Americans exploded a small atom bomb in one cavern, the force of which was aimed at a target of deuterium/tritium pellets in another, contiguous cavern some distance from the atomic bomb: they filtered the thermal energy produced as much as possible with opportune heat exchangers in an effort to concentrate the atomic radiation on the target in the second, contiguous cavern. The atomic radiation, consisting of gamma rays and neutrons which preceded the thermal wave to the target, produced nuclear fusion—which was accurately recorded by sheltered instrument sensors in another area. Immediately after detonation, the thermal wave destroyed all the instrumentation, but the experiment was complete and the results recorded. Nobel Prize winner Carlo Rubbia confirmed this experiment in an interview with the Corriere della Sera on May 13, 1988. I believe that this experiment also completely confirms the correctness of my ideas.

G) Cold Fusion: Fleischmann, Pons, and Jones
March 24, 1989; March 24, 1989

Everyone knows about the recent ups and downs of these researchers (who I mentioned at the beginning of this update), and I need not dwell on them further.

H) Rome, Italy, News Conference March 31 1989; March 31, 1989

During this news conference, for an entire morning, in front of numerous journalists and researchers, I discussed the Sun's cold nucleus and also the super-cold temperatures at which, in my view, a fusion reaction was possible. The press agent for ENEA was also present among the other journalists.

I) F. Scaramuzzi and ENEA's Cold Fusion Experiment
April 27, 1989; April 17, 1989

Without wanting to level malicious insinuations, I find it truly singular that—according to Scaramuzzi himself—ENEA, he, and other collaborators came up with the idea of cryogenic fusion on the very same day as my Rome news conference. In any event, I am the first to recognize that between the cold fusion that I hypothesized and that of ENEA's, the only thing in common is the use of liquid nitrogen as the refrigerant to produce the cryogenic state. There was no reason, therefore, for Professor Scaramuzzi to declare to TG2 (on April 18, 1989, at 7:45 P.M.) that no cryogenic theory formed the basis for his experiment—even though, however, without a strong thermal variation from -196°C up to ambient temperature, no sign of nuclear fusion is noted in ENEA's experiment.

J) Enrico Fermi's Experiment April 10, 1989; July 31, 1937

Nobody, either in Italy or elsewhere, knew about a bi-monthly publication entitled *Scientific Research and Technical Progress in the National Economy*—except two researchers from Verona, Professor Speri and Architect Zorzi, who in 1967—by pure chance—found at a used book stand Issue No. 1-2, Series II, Volume II, Year VIII, July 15-31, 1937. In this issue of the magazine—photocopies of which were kindly forwarded to me by the above-mentioned Speri and Zorzi—is an article, on pages 40-43, signed by E. Amaldi, E. Fermi, and F. Rasetti of the National Committee on Physics, entitled, "An Artificial Neutron Generator."

This work by Fermi and his collaborators describes a system for neutron production via deuterium ions which, while accelerated to only 200 KV, were targeted on a compound rich in deuterium. The yield obtained at that time—the article summary reports—was equal to that of a positive beryllium source emanation of 2.5 Curies.

The apparatus consisted of a glass vacuum tube connected at one end to an Oliphant-type channeled deuteron-ray source. The deuteron rays, accelerated by a tension of only 200 KV, were directed at a target of heavy ice (frozen heavy water, cooled by liquid air) at the other end of the tube.

The nuclear reaction utilized (written clearly in letters) is as follows:

$$ {}^{2}_{1}\mathrm{D} + {}^{2}_{1}\mathrm{D} = {}^{3}_{2}\mathrm{He} + {}^{1}\mathrm{n}_{0} \quad (+3.2\ \mathrm{MeV}) \qquad (1)$$

This reaction is a true cold fusion reaction obtained at -194°C. It is therefore a cold fusion in spite of any objection that thermofusionists can raise. That Fermi did not have nuclear fusion as his objective, but instead only the artificial production of neutrons, is demonstrated by the following fact:

along with reaction 1), also normally taking place—with sensibly equal probability—is:

$$ {}^{2}_{1}D + {}^{2}_{1}D = {}^{3}_{1}T + {}^{1}_{1}p \qquad (+4.0\ \mathrm{MeV}) \qquad (2) $$

Now, given the considerable deuterium flux through the apparatus, a recovery system is necessary. In this regard, the deuterium gas, after being pumped, is collected in a gasometer instead of being discharged into the air, and from here is filtered through a barrier of incandescent palladium to filter out any impurities with which it might have come into contact. This purified gas is recycled through the valve core to the deuteron source. It's therefore evident that Fermi was interested only in recovering the deuterium, disregarding other eventual reaction products as well as reaction 2) which, in fact, produces no neutrons.

The fact that Fermi's experiment produced a large quantity of neutrons indicates without a shadow of a doubt that reaction 1) occurred not only at the low-energy level involved, but above all in contrast to the barrier of potential, the enemy which thermofusionists continually, in every conceivable way, try to defeat.

It's known, in fact, that all the efforts to greatly elevate (above 100 million ˚C) the temperature of the plasma containing deuterium nuclei or the mixture of deuterium/tritium—which are all positively charged and which therefore would repel instead of attract each other—require the surrounding of this electrostatic barrier with heat in order to confer on the particles that must be forced together such kinetic energy as is sufficient to allow d - d or d - t collisions, and therefore their fusion. Fermi's experiment—of which I became aware after fifty-two years had passed (!)—is by itself sufficient to silence those who up to our time have impudently denied the possibility of cold fusion. It is, therefore, an irrefutable and shining confirmation of my ideas as expressed in my "Note on 'Thermo' Nuclear Fusion."

K) Hydrogen and Hydrogen-Isotope Based Controlled Thermonuclear Energy Producer (Patented by Speri)
April 10, 1989; September 30, 1974

The two Verona researchers, Professor Homer Speri and Architect Pierre Zorzi, after having fortunately discovered Fermi's above-cited article in 1967, think that they can obtain nuclear fusion in a way unlike those of ultra-cold heavy ice or ultra-high temperatures (on which physicists worldwide are feverishly working). With their apparatus applied to a normal combustion engine and supplied with extra electrodes which produce high tension scintillas,* they are able—as described in their patent dated September 30, 1974—to improve the yield of the engine an incredible amount (on the order of 30% to 40%).

According to Speri and Zorzi this would be due to thermonuclear fusion of hydrogenated products in the fuel or added to it. But it is less easy to perceive, even considering Speri's declarations, the possible connection between a cold fusion phenomenon (Fermi) and a thermofusion phenomenon. And, according to Speri's presentation in May of 1991 to the International Convention at Ischia, the phenomenon of electrical energy is responsible for pooling the two types of fusion, hot and cold. This explanation seems insufficient to me, even if I am convinced that the tremendous temperature calculated to be at the center of a scintilla (10^{40}°C) could actually cause this fusion.

You might think that I would be cutting the ground out from under my own feet if I—who have been sustaining for over thirty years that cold fusion is a natural phenomenon and therefore to be copied, as opposed to a type of fusion, thermofusion, which I believe to be unattainable—were to admit the possibility that the scintillas of Speri and Zorzi's apparatus had produced thermofusion. On this subject I must be more precise. In my preceding "Note on 'Thermo' Nuclear Fusion" I began to speak of an internally cold and externally hot sun; and I spoke of nuclear phenomena which in my opinion happen in all stars like ours and therefore are natural phenomena. It seemed coherent to me, and, therefore, logical and consequential to suggest the "cold" route to nuclear fusion, negating the fact that the thermofusionists could be on the right track with their "Tokamaks" and similar mischief (which are truly only cathedrals in the desert). However, without searching for cosmic proof of cold fusion, it seems that the two "earthly" experiments—Fermi's and the one in March of 1986 in Nevada—are valid and extremely eloquent proof of my point of view concerning fusion.

If the thermofusionists then want to "run" with Speri and Zorzi's "ball on the rebound," then it will be necessary to closely examine the frightening discrepancy that exists between the highest temperature obtained in more than forty years of extremely costly attempts in their Tokamak-type equipment—two-hundred paltry million degrees centigrade (2×10^{8}°C)—and the temperature at the "heart" of an electric scintilla which under optimum conditions can reach, as I've noted above, 10^{40}°C, and which is therefore 5×10^{31} times higher. If they want to continue on the "thermal" route, without casting by the wayside neither magnetic confinement fusion nor potent laser inertial confinement, they should at least try to work on electric discharges, keeping in mind, however, that this too is an uphill battle that must be started over again.

And now, before ending this update on my 1984 "Note on 'Thermo' Nuclear Fusion," I would like to point out that the experiments of Fleischmann, Pons, and Jones have resurrected the names of other forgotten scholars who never had the good fortune to witness affirmation of their ideas on cold fusion. I will cite them chronologically:

L) German chemists, Paneth and Peters (March 14,1990; 1926) who, in 1926, while performing electrochemical experiments with deuterium water produced helium.

M) The Farkas brothers of Cambridge who in 1935, while infusing deuterium gas into a palladium tube, produced hydrogen at the tube's exit. Ernest Rutherford, on being consulted, peremptorily claimed that it was impossible.

N) The Soviet, Alikin, who in 1971 announced that he had achieved cold fusion, but was silenced by the Artsimovich Science Academy.

At this point I cannot leave out C. Louis Kervran's studies and publications on low energy transmutations. Nor can I fail to recognize Sir Frederik William Herschel's (1738 - 1822) work in astronomy and astrophysics, the precursors of the theories on the "cold" core of the Sun: he has been derided and scorned even in our present time!

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*sparks is alternate translation from the Italian even in our present time!

Unfortunately, man is afraid of new concepts. He rebels at the idea of "rummaging in his own pockets" for fear that he might discover some truths that will contradict his convictions. But, meanwhile, huge lies continue to circulate undisturbed and are handed down from teacher to student like wandering mines in the archipelago of knowledge.

To conclude this update to my "Note on 'Thermo' Nuclear Fusion," there is little news on my own research. Not having at my disposal equipment to produce gamma rays and neutrons to aim at deuterium and lithium targets (which I also don't have . . .), I have re-begun with Fermi's experiment, modified to operate at ambient temperature, and am attempting to contemporaneously obtain neutrons and gamma rays inside an activated deuterium target, hitting it with moderate energy deuteron beams.

For now I have obtained particle emissions (probably neutrons) and anomalous heat which continues for awhile even after the equipment is shut down. If promised control equipment doesn't arrive on loan, and if I can't find a little more deuterium and heavy water, I will be forced to shut down my laboratory . . .

I am in fact working alone, practically without help of any kind, with a paucity of truly disheartening equipment. To continue, at seventy-two years of age, a work this demanding under the present circumstances is sheer folly, and I don't know how long I'll be able to hold out!

References

1. Carlo Matteucci—Italian—(1811-1868): Mathematician, chemist, physiologist, physicist; esteemed researcher, prolific author of various scientific works; Professor of Physics at the University of Pisa; Senator, Minister of Public Instruction, etc.

2. This noise is familiar and unmistakable to any who have used a rotating basin or drum for mixing or confectionery purposes: *e.g.*, agricultural seed-sowing, an activity I have enjoyed and practiced in Italy for almost twenty years, from 1967 to 1986.

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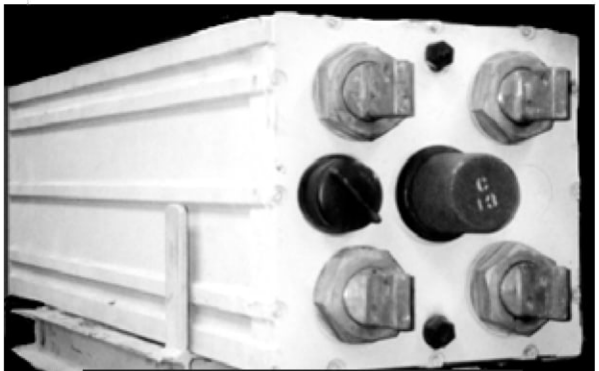

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Three Decades of Cold Fusion Prior to Pons and Fleischmann

Peter Graneau* and Neal Graneau**

We concur with Chubb[1] when he attributes the cold fusion furor in large measure to language. Pons and Fleischmann must bear some of the responsibility for not knowing that cold fusion was actually discovered in the years between 1955 and 1958 in the Berkeley and Harwell laboratories. Both laboratories made it very clear that the neutrons which they detected were fusion ashes and could not possibly have been produced by thermal collisions. Thermal action creates disorder. What was discovered in the 1950s was that the neutrons all flew in the same direction. If the experiments had generated high temperatures, below the fusion threshold, this would have been a hindrance to any ordered forces which tried to bring about nuclear collisions. Hence one could have called it cold fusion. These facts are well-documented, not disputed, easily confirmed by reproducible experiments, and they were funded by two governments.

In 1958 a team of fusion researchers under Baker,[2] at the Berkeley Radiation Laboratory, reported neutron production in electromagnetically pinched deuterium gas. It turned out to be the first major disclosure of non-thermal fusion reactions. The research was performed within the U.S. program of controlled fusion, code named "Project Sherwood,"[3] and funded by the United States Atomic Energy Commission. Declassification of this project in 1958 led to the paper by Baker and his colleagues. Large numbers of neutrons, representing nuclear ashes, had been detected in deuterium pinch experiments as early as 1955. Electricity generation by neutron heating of water and steam generation seemed to be around the corner. The Berkeley experiment was simple and did not involve complex and expensive apparatus. Progress apparently was as fast as it had been in nuclear bomb research. But by 1958 the nuclear physicists had discovered that the crucial reactions must have been cold fusion and this was deemed to be a great disappointment for, it was thought, a continuously functioning nuclear reactor would have to start "burning" spontaneously once it was "ignited." This was only possible with hot fusion.

The members of the Sherwood project went to great length to prove that the neutrons could not have been created by thermonuclear reactions. They listed no less than ten different reasons, presumably in decreasing order of importance. The first evidence for cold fusion was cited to be, primarily, the axial expulsion of neutrons from the pinch column. Thermonuclear collisions are known to produce an isotropic distribution of expelled neutrons because of the random thermal motions of the deuterons.

The second stated indication of non-thermal fusion was that neutron production was quenched by the application of a weak axial magnetic field (50 - 100 gauss), implying that collisions were dominated by electromagnetic rather than thermal forces. The third point was that the neutron yield did not rise when the applied voltage was increased. The list continued, concluding with reason ten, which was that neutrons were produced at points all along the pinch column, proving that they were not created by the voltage across the full length of the tube.

Bishop,[3] chief of Project Sherwood, summarized the results of pinch tube experiments as follows:

> Two bits of evidence were accumulated that could not be reconciled with the theory of thermonuclear origin. In the first place, the number of neutrons observed was too great, under the operating conditions of the experiments; the temperatures predicted from the Rosenbluth theory were too low to produce so many neutrons from fusion reactions. The second and even more convincing evidence was the result of a careful study of the energy spectrum of the neutrons which were emitted. This study, carried out initially at UCRL, Berkeley, showed that while the neutrons were coming from the body of the discharge, the deuterons responsible for their production (through D-D reactions) were unquestionably moving with rather high velocities in the *axial* direction. The deuterons, therefore, did not have random velocities, as required for true thermonuclear conditions. Instead, they had somehow acquired axial velocities greater than they would have achieved by being accelerated the entire length of the tube! For example, with only 20 kV applied across the tube, the deuterons responsible for producing the neutrons were found to have an average energy of the order of 50 kV.

At the same time, toroidal pinch experiments were performed in Britain with a machine called ZETA. Like the linear pinch tubes in Berkeley, ZETA also generated large numbers of neutrons, which were immediately hailed as a success in controlled fusion. In April 1958,[4] the British scientists admitted that their neutrons were not of thermonuclear origin.

. . . cold fusion was actually discovered in the years between 1955 and 1958 in the Berkeley and Harwell laboratories.

It is a puzzle why the experts treated the cold fusion findings as a disappointment, because, as we now know, the temperature requirements of hot fusion are very difficult to satisfy. Somehow it was argued that sustained fusion power generation would require a nuclear "burn" once it was "ignited." The axial emission of neutrons from the pinch column required axial acceleration of the deuterons, and increased voltages between the electrodes were tried, but they did not improve the neutron yield. The application of an axial magnetic field did stop the fusion reactions, and this should have been the clue to what was driving the deuterons. Later this led us to propose that the deuteron ions were accelerated by longitudinal magnetic Ampere forces.[5] We called it filament fusion. Unlike the term cold fusion, it stirred no interest in the subject. Longitudinal Ampere forces were unknown to fusion researchers in the 1950s.

Nevertheless, cold fusion research continued with large pulses of current through pinched deuterium gas (plasma focus fusion),[6] heavy water filaments (capillary fusion),[7] and solid deuterium filaments.[8] Government funding was made available for this purpose in the United States, Britain, Germany, and Italy. The collective cost is not known, but it could have been as high as $100 million. This widely published research was dropped only a few years ago in the United States and in Britain, under budgetary pressure exerted by the hot fusion community.

Shortly after the publication of our *Physics Letters A*[5] paper on the role of Ampere forces in nuclear fusion, Chappell Brown[9] reviewed our speculations in 1992. He solicited the opinions of two prominent fusion researchers. One was Professor Haines of Imperial College, London, and the other was Anthony Robson of the Naval Research Laboratory, Washington, D.C. Both were responsible for experimental investigations of fusion reactions produced by current pulses through about 10 cm-long pieces of what Brown called frozen deuterium wires. We argued, as Lochte-Holtgreven[7] did, that what explodes ordinary copper wires into many short pieces[10] also produces the deuteron collisions.

Haines and Robson admitted that their deuterium wires broke up into short pieces and the neutrons could not have been the result of thermonuclear reactions. They estimated that the temperature might have been of the order of one million degrees whereas hot fusion requires at least 100 million degrees.

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About the Authors

Interspersed with industrial research on novel power lines, Dr. Peter Graneau devoted thirty years of his career to fundamental issues of electromagnetism and inertia. Many of his critical experiments were carried out at the Massachusetts Institute of Technology. He published some one hundred papers in physics and engineering journals which were summarized in four books.

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Dr. Neal Graneau participated in a number of the MIT experiments and was co-author of the two books: *Newton Versus Einstein* and *Newtonian Electrodynamics.* He studied physics at King's College, London—where Maxwell wrote his equations—and plasma physics at Oxford University. He is now heading a laboratory in Oxford which is dedicated to the liberation of clean renewable energy from water. The beginnings of his Newtonian theory of light will be published in the forthcoming book *Action-at-a-Distance: Pro and Contra.*

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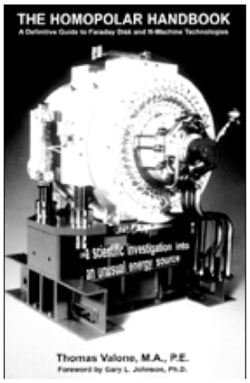

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Thomas Valone, M.A., P.E.
Foreword by Gary L. Johnson, Ph.D.

Is the Occurrence of Cold Nuclear Reactions Widespread Throughout Nature?

John O'M. Bockris* and Eugene F. Mallove**

Introduction

The generally accepted idea is that the paper of Fleischmann, Pons, and Hawkins of 1989[1] gave the first evidence that nuclear reactions occur in the cold in a palladium-deuterium (Pd-D) electrolysis system. However, published accounts on elemental change in solids have been around since before the discovery of the nucleus (Rutherford[2]). Thus, the earliest evidence for creation of new elements was that reported for reactions in biological systems. In view of the wave of negative opinion against the 1989 and thereafter work, it is interesting to note a U.S. Army report of 1978[3] which concluded that the evidence for nuclear reactions in biology was strongly positive. Even less well-known is that U.S. government labs in the 1970s[4] experimented with neutron emission arising from the passage of very high currents through wires, conditions under which thermonuclear reactions should not have been occurring. [Editor's Note: See also P. and N. Graneaus' article, pp. 27-28 in this issue of *Infinite Energy*.]

In this paper, facts will be cited as evidence that nuclear reactions occur not only in Pd-D systems but in many parts of Nature. Our present chemical interpretation of natural phenomena—which neglects nuclear activity in the normal ambient temperature world (far less than millions of degrees K)—is incomplete. Though not all the following cited phenomena may turn out to be real—and some are certainly more likely to be than others—this listing is an indication of how wrong may be the present paradigm, which denies low-energy nuclear reactions, reactions "in the cold."

Hydrogen in Palladium

Fleischmann, Pons, and Hawkins[1] observed substantial heat of nuclear reaction magnitude (*cf.* the conclusion of the Army's 1978 report) and a low-level emission of neutrons. Tritium, measured by an indirect method, was also claimed.

The material on D-Pd is summarized in the book by Kozima.[5] However, the production of neutrons by Borghi[39] from a klystron discharge, and other chemical work cited below, and the biological work, preceded the much quoted Fleischmann and Pons work. Further, there is some evidence that others, such as Paneth, Peters, and Tandberg, in the 1920s and 1930s may have observed low-energy nuclear reactions in palladium metal hydride systems—even though the first two authors later rejected their initial nuclear conclusions.[41]

Discussion of systems using H instead of D began in 1989 among electrochemists, for it was seen, experimentally, that the tunneling probabilities for H greatly exceed those for D. The first publication involving H as opposed to D seems to have been that of Mills and Kneizys[6] in 1991.

Enyo and Ohmori[7] reported heat equivalent to four times the input energy using Pd and an aqueous system. Mizuno[8] showed that a large excess of heat from ceramic proton conductors can be obtained, but the reproducibility was poor. R. Oriani at the University of Minnesota later confirmed the Mizuno proton conductor work.[42]

The most remarkable development of H systems has been in the Patterson cell,[9] which involves an array of plastic microspheres, plated with a thin film of Cu, Ni, and Pd. Heat up to 1,000 times the electrical input power has been recorded. The results of testing Patterson cells by the Motorola Corporation also substantiated the so-called "heat-after-death" phenomenon, in which nuclear-scale power generation was seen to persist for days even after input electrolysis power was cut off. This general phenomenon has been seen by Fleischmann and Pons[43] and by Mizuno.[22]

The Discovery of He-4 in Electrolysis of LiOD in D_2O

When D_2 is evolved from D-Pd on Pd, one of the possible nuclear products is He-4. This was first reported present in the evolved D_2 by Melvin Miles *et al.*[31] in 1991. However, doubts were expressed because the system first used was of glass and diffusion (from the outside air) of He-4 through glass was suspected. Miles *et al.* remain firm in their opinion that helium evolution roughly commensurate with excess heat production has been demonstrated then and subsequently.[32,34,44]

In 1992, Chien *et al.*[30] discovered He-4 in Pd after prolonged electrolysis (Pd-LiOD-D_2O). Pd maintained for long periods in air contains about 10^9 atoms cc^{-1} of He-4. After electrolysis, the amounts were 10^{10} - 10^{11} atoms cc^{-1}. The experiments eliminated the possibility that the extra He-4 is from the air.

The seminal work of Arata and Zhang[45] with the Pd-D system, finding massive nuclear scale excess energy (up to 500 MJ/mol) as well as He-4 and He-3 in anomalous (non-natural) ratio, lend considerable support to the earlier helium detections.

Alchemical Experiments at Texas A&M

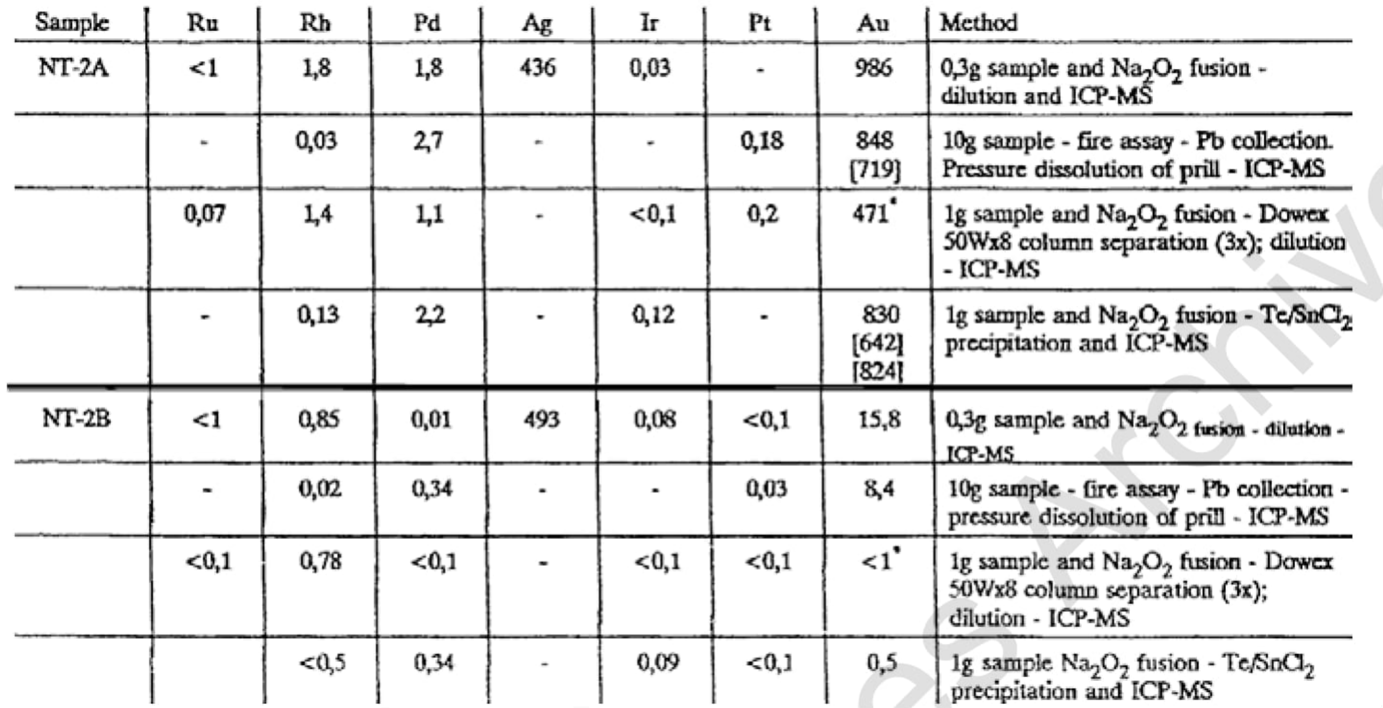
Joseph Champion, an electronics technician with experience in work related to the electrochemical extraction of metals from sea water, approached the author Bockris at Texas A&M University in 1992 with claims to have used a mechanical impact method to produce Au from Pb and Hg salts. His method was examined, the practical work being done by Bhardwaj and Lin.[11] The starting material contained Hg, Pb, S, graphite powder, and KNO_3. Ignition of this mixture causes a mild explosion. After two to three days, the resulting mixture was examined by several methods and by four independent analytical agencies. Some results from one of four successful experiments are in Table 1. Au at up to 300 ppm (Table 1) was detected in the mixture (original mixture <1ppm). The method gave good results only sporadically. A β radiation activity was discovered in the product.[28] The work was curtailed in March 1993 because of difficulties in funding. Restrictions due to the sponsor's wish for confidentiality prevented publication of the Texas A&M results until 1996.[11]

A reproduction of the production of noble metals by Champion's impact method has been reported by Cau.[12] Correspondingly, Filiminov and Kobets[13] have shown that an impact method can be used to cause change in the isotopic abundance in Cs, *i.e.*, a nuclear reaction is made to occur.

This work of April 1992 at Texas A&M (see Table 1) is noteworthy because it formed the beginning of the era of scientific work on *transmutation* of metals in the cold, now the subject of sessions held in recent years at meetings of the American Nuclear Society and the American Physical Society.

Champion's low level of scientific qualification (and checkered career) prevented credit being given to him. Along with the Italian physicist, Roberto Monti,[14] he was the pioneer (in 1992)

Table 1. Champion Impact Method Tested by R. Bhardwaj and G. Lin at Texas A&M University (1992). Results in p.p.m.



Sample	Ru	Rh	Pd	Ag	Ir	Pt	Au	Method
NT-2A	<1	1,8	1,8	436	0,03	-	986	0,3g sample and Na_2O_2 fusion - dilution and ICP-MS
	-	0,03	2,7	-	-	0,18	848 [719]	10g sample - fire assay - Pb collection. Pressure dissolution of prill - ICP-MS
	0,07	1,4	1,1	-	<0,1	0,2	471*	1g sample and Na_2O_2 fusion - Dowex 50Wx8 column separation (3x); dilution - ICP-MS
	-	0,13	2,2	-	0,12	-	830 [642] [824]	1g sample and Na_2O_2 fusion - Te/$SnCl_2$ precipitation and ICP-MS
NT-2B	<1	0,85	0,01	493	0,08	<0,1	15,8	0,3g sample and Na_2O_2 fusion - dilution - ICP-MS
	-	0,02	0,34	-	-	0,03	8,4	10g sample - fire assay - Pb collection - pressure dissolution of prill - ICP-MS
	<0,1	0,78	<0,1	-	<0,1	<0,1	<1*	1g sample and Na_2O_2 fusion - Dowex 50W/x8 column separation (3x); dilution - ICP-MS
		<0,5	0,34	-	0,09	<0,1	0,5	1g sample Na_2O_2 fusion - Te/$SnCl_2$ precipitation and ICP-MS

of helping to break through a thick wall of prejudice against the occurrence of metal-to-metal transmutation in the cold.

The Results of Arcing in Gases and Liquids

Little known experiments on the effect of sparking in octane-air mixture were carried out in 1974 by Speri and Zorzi[15] though not published until 1989.[24] The heat produced by chemical combustion of octane-air was measured. Then, three extra sparking units were added to the system and a significant increase in heat evolution was observed over that calculated from the chemical reaction. The authors show calculations on the assumption that the D naturally present undergoes fusion to form He-4, whereupon heat would be evolved. If one naturally occurring deuteron per thousand present reacts, they achieve numerical agreement with their results.

Yull Brown[16] added D_2O to an aqueous alkaline solution and ignited the gaseous H_2-HD-D_2O mixture resulting from electrolysis. He claimed a flame reaching 6000°C. Tungsten was easily melted and sublimed. Tritium was found in the water condensate. However, further observations of the so-called "Brown's gas" at NERL in 1998 cast doubt on whether any anomalous "sublimation" of tungsten really does occur in any Brown's gas experiments (though the NERL tests were not done with D_2O). More likely, the tungsten sublimation is mere oxidation of tungsten—the same process that occurs with a normal stoichiometric oxy-hydrogen torch, as demonstrated at NERL.

Roberto Monti stressed an "alpha particle structure" of the nucleus and on this basis predicted transmutation in the cold. He presented his ideas in correspondence to Bockris in 1990, and subsequently described an unpublished experiment in which Fe had been observed to be formed from carbon if an arc was struck between two pieces of pure carbon immersed in water.

This experiment was performed by R. Sundaresan[17] in Bockris' laboratory in 1993 with meticulous attention to impurities in the spectroscopically pure carbon, and in the water and ambient air. A 10 amp current between two spectroscopically pure carbon rods, in highly purified water, passed for ten hours, produced small quantities of Fe. If O_2 were excluded, no Fe was formed. The following nuclear reaction (suggested by G. Lin) is quantitatively consistent with the observation:

$$2\,{}_{6}C^{12} + 2\,{}_{8}O^{18} \rightarrow {}_{26}Fe^{56} + {}_{2}He^{4}$$

Many Metals from H and D in Pd

The late Kevin Wolf,[18] at Texas A&M University, found a number of radioactive elements in Pd after prolonged electrolysis of LiOD in D_2O. The discovery was made in September of 1992 (*cf.* The Champion impact experiments at Texas A&M in April, 1992). Wolf had rejected the finding of tritium at Texas A&M in 1989 in spite of 1990 work in which he had reported finding tritium[19] in his own experiment. Thus, his clear findings of the product of transmutation was a result inconsistent with his stance in respect to tritium. He chose not to publish the transmutation evidence nor to present the work at meetings such as ICCF-4 in Hawaii, though invited to do so. [Editor's Note: See the account of this astonishing episode in *Infinite Energy*, No. 2, 1995, "Alchemy Nightmare."]

Wolf's transmutation work was finally published by Tom Passell,[18] a project leader in the institution (EPRI) which had paid for this cold fusion research work. The first formal publication of Wolf's work on tritium was in 1996.[19]

However, in 1995, Bockris and Minevski[20] published the results of new materials they had found in Pd after electrolysis for three weeks. Among the new metals identified by EDS were Al, Mg, Cl, Ca, and Fe. Bockris and Minevski made parallel studies of the solution by ICP and found there certain impurities (*e.g.*, Pt, Si and Zn) which did indeed show up in the electrode surface and were detected by XPS (which has a penetration depth of ~30 Å).

The technique used in the Bockris and Minevski work, of measuring surface impurities (by XPS) and then impurities at various depths (by EDS), overcomes the criticism that the new materials come from the solution. This work, recorded from the 1995 thesis of Zoran Minevski,[35] preceded the work of Miley *et al.*,[21] and

Mizuno *et al.*,[22] who repeated experiments using isotopic abundance ratios to distinguish new material. However, they did not do the double analysis of the surface and bulk; nor did they compare with impurities in solution. They were dependent on the (sometimes ambiguous) isotope ratio studies to prove the transmutation.

Diminution of Radioactivity

There have been several claims to be able to do this. The first came from Yull Brown[36] and was tested in front of DOE engineers who are reported to have said that describing what they had seen in these reports could threaten their employment by DOE. Hence, they would be forced to report a null finding. Brown's method was to play the intense flame from the so-called "Brown's Gas" (a hydrogen-oxygen mixture, probably containing HD) on a sample of radioactive wastes. Diminution of the radioactivity by about 80% in fifteen minutes was claimed. Tests were made to ensure that no material from the container had escaped to the laboratory, or that the original radioactivity was not blocked inside large particles.

Stan Gleason,[37] working with colleagues calling themselves jointly The Cincinnati Group, subjected a solution of $Th(NO_3)$ to AC electrolysis and claimed diminution of the radioactivity of the solution by some 90% in some hours. A highly anomalous ratio of copper isotope abundances in the *visible* fragment of copper *produced* in the cell was observed. Fox[38] reported similar radioactivity diminution results and examined deposits formed on the electrodes and in the cell to ensure that the original $Th(NO_3)$ was not contained there. F. Celani[49] in Italy has provided partial confirmation of The Cincinnati Group's work using one of that group's tests cells, which are being marketed as a demonstration cell—unfortunately with few purchasers. This is due, no doubt, to the very heretical nature of the work as well as the need for good nuclear measurements that are generally not available to many prospective researchers.

Detailed descriptions of these efforts have been published in *Infinite Energy* magazine and the *Journal of New Energy*, but no further reports can be given on these results because of the paucity of details published, perhaps due to patent concerns. However, as of June 1999,[33] studies of the effect Brown's gas are being carried out in the Nuclear Facility at Chalk River in Canada, and at the University of Ottawa and in Ireland under European Commission funding. There have also been tests of Clean Energy Technologies, Inc. (CETI) radioactivity remediation cells at a DOE contractor's laboratory in the state of Washington. Results of these tests are said to give positive indications of low-energy transmutation, but details are currently not available.

Perhaps the most provocative development in the field of Low-Energy Nuclear Reactions is the award of a DOE contract in May 1999 to the Low-energy Nuclear Reactions group of Professor George Miley at the University of Illinois, Champaign-Urbana (see *Infinite Energy*, No. 26, p. 43). The purpose of this peer-reviewed award is to demonstrate the electrolytic "cold fissioning" of radioactive elements such as uranium and thorium, precisely what Prof. Miley as well as CETI have already demonstrated independently to their mutual satisfaction. Naturally, political and scientific opposition to this award for heretical investigation has already arisen, a controversy that has now boiled over into the general scientific press (see *Science*, July 23, 1999, 505-506 and an editorial in this issue, p. 4).

Nuclear Reaction in Chemical Systems

It has been seen that early claims about nuclear effects in chemical reactions were made by Speri and Zorzi in 1978[15] and, much earlier, in biological systems.[3, 10, 25]

Some surprising work has been carried out by L. C. Case[23] which seems to establish cold nuclear reactions in chemistry in a very desirable way. He confined D_2 over Pd-doped carbon at modest pressures and $130 < T < 275°C$. 100 ppm He-4 was detected in the resulting gas. This helium-generation in what Case calls "catalytic fusion" has now been confirmed in multiple studies performed at SRI International (see *Infinite Energy*, No. 26, p. 16). Independent efforts by Saturna Technologies, Inc. and at NERL are under way to further confirm and develop the Case studies. Case himself is aiming to produce a self-sustaining catalytic heat generator (self-heating with no input power) that will also produce helium.

Remarkable results (which have been verified by three independent German laboratories) have been obtained by Mündt, as reported by Petermann.[40] Mündt has measured the input and output energy in the anaerobic combustion of trash. The necessary initial heat input is provided by household light bulbs. The vessel is built to withstand pressures formed by the products of the reactions occurring among the trash in the absence of O_2, but as a result of heating. The thermal output is 2.7 times the input. As with the Champion and Wolf experiments, radioactivity was detected. The development of a magnetic field is also reported.

Biotransmutation

The anomalous formation of new nuclei has received quantitative examination since 1799 when Vauquelin[24] observed a large quantity of lime (CaO) in the excreta of chickens. He fed the chickens only on oats, measuring the Ca in the oats and then in the chicken excreta and eggs. He found twelve times more Ca in the chickens' output compared with the amount of the input. In 1960, Professor Baranger[24] examined germinating seeds for P and Ca and concluded that the plants manufactured new atomic species.

The most well-known and extensive works in this field are those of Kervran, and on this basis, he was proposed for the Nobel Prize.[25] His work was examined by a committee of the U.S. Army Materials Technology Laboratory, which published a report in 1978 entitled "Energy Development from Elemental Transmutation in Biological Systems." The report concluded that transmutation took place at a cellular level (see reprint in *Infinite Energy*, No. 3, 18, p. 78). The Army report included a proposed mechanism. Mg ATP was considered to be a microcyclotron. When Mg ATP molecules are placed in layers, one on top of the other, the authors thought they could be seen as having the properties of a cyclotron.

Very remarkably, this 1978 report by an established committee has been ignored (as was the U.S. government work on the successful production of neutrons from passing electricity through wires). The work of H. Komaki in Japan[46] and V.I. Vysotskii *et al.*[47] in the Ukraine and Russia has also supported the remarkable paradigm of nuclear transmutation in biological systems. The more recent cold fusion and low-energy nuclear reaction work following the Fleischmann-Pons announcement in March 1989 has generally been viewed as a new development, unconnected with past research.

M. Puri,[26] a theoretical physicist, has reported some experiments on the germination of ryegrass seeds. The seeds were germinated for twenty-nine days. Evian water (which was analyzed for K, Mg, and Cu) was added (see Table 2).

Puri also reports experiments on the Ca in hens eggs. The normal diet of hens does not contain enough Ca for one egg. If the hens were kept away from Ca-containing foods, they laid eggs with thin shells devoid of Ca. If K (element 19) was added to the diet, the hens laid eggs with thick shells containing Ca (element 20).

Table 2. Results of Puri's Experiment

	Before germination (milligrams)	Change after germination (with grams) (After correcting for Evian water)
K	6.97	+ 9.31
Ca	6.00	- 2.61
Mg	10.32	-10.14
Cu	0.021	+ 0.079

Table 3. Chronological Summary of First Observations of Cold Nuclear Reactions in Various Systems

Phenomenon	When Firt Observed	Date of First Publication
Vanquelin, chicken excreta	1799	M. Kushi, *The Philosopher's Stone*, One World Press, 1994
Baranger, P and Ca in germinating seeds	1960	As Above
Kerveran, numerous biological observations of elemental transmutation	1966	L. Kerveran, *Biological Transmutation*, Beckman, NY 1971
Komaki, experimented on the content of P, K, and Ca in bacteria, compared with the removal of each of these successfully in nutrients	1967	I. Komaki, *Rev. de Pathologie Comp.* 67 (1967) 213
Kushi, Na → K; K to Ca; Mn to Fe	1967	M. Kushi, in book quoted above
U.S. Government Labs, neutrons from high currents through wires	1970	P. Graneau and N. Graneau, *Newton vs. Einstein*, Carlton, NY, 1994
Speri and Zorza, interpretation heat increase obtained in combustion if mixture sparked	1974	H. Bertocci and T.P. Wesh, Ref. 15
U.S. Government reports	1978	"Energy Development from Elemental Trammate in Biological Systems," U.S. Army Materials Technology Laboratory 1978, quoted by M. Kushi, ref as above
Fleischmann, Pons, and Hawkins, heat and neutrons from Pd-LiOD electrolysis	1989	M. Fleischmann, S. Pons, and M. Hawkins, *J. Electroanalyt. Chem.*, 267 (1989) 301
Packham *et. al.*, Direct measure of tritium from Pd-LiOD electrolysis	1989	N.J.C. Packham, K. Wolf, J.C. Wass, R.C. Kainthla, and J. O'M. Bockris, *J. Electroanalyt. Chem.*, 270 (1989) 451
Appleby and R. Bush, pure Li^6 solution gave same results as natural Li	1990	Ref. 29
M. Miles and R. Bush, He^4 in gas phase following	1991	R.E. Bush, T.J. Lagowski, M.W. Miles, and G.S. Osi, *J. Electroanalyt. Chem.*
Murphy, Li 4000 Å deep into Pd	1990	Presented ICCF-1, 1990
Monti, in letters advocated general occurrence cold nuclear reaction based on a particle theory of nucleus, Brown's gas, played on radioactive waste → diminution radioactivity	1990	Letters to JOMB, 1990
Chien *et al.*, He^4 found in Pd after electrolysis in D_2O	1992	Ref. 30
Reifenschweiller, heat reduces activity of tritium	1966	O. Reifenschweiller, *Phys. Letts. A*, 184, (1994) 149
Champion, impact method gives Au and noble metals from Hg and Pb	1992 (April)	Ref. 11
Long, γ emission from gas discharge tube	1992	H. Long, ICCF-3, Nagoya, p. 447
Notoya, γ emission from electrolysis of LiOH-Pd	1996	R. Notoya, T. Onnishi, and Y. Noya, ICCF-6, Hokkaido 1996, 675
Gleason, a.c. electrolysis of $Th(NO_3)$ gives diminution of radioactivity	1995	
Bockris and Sundaresan, C arc → Fe in D_2 containing H_2O	1994	Ref. 17
Bockris and Minevski, electrolysis of LiOH on Pd → numerous new metals	1993	Ref. 20
Speck, X-ray emission from Pd in LiOD-D_2O	1996	
Mundt, compressed trash, heated anaerobically → excess heat and radioactivity	1997	Ref. 26.

Speculative Interpretation of "Inedics"

There have been reports of persons ("inedics") who eat nothing but a daily communion wafer yet live for many years, apparently in excellent health, while performing daily manual labor. In skeptical modern times, nurses have lived with some of these for two weeks and have certified that no food in addition to the thin wafer eaten at communion was consumed.[27]

In view of the establishment by French, Japanese, and Indian workers of nuclear transmutation in biological organisms, may it be that, in the absence of enough food for chemical metabolism, there are rare cases in which a nuclear mechanism of metabolism comes into operation. Thus, water vapor, CO_2, O_2, and N_2 (*i.e.*, the elemental constituents of fats and proteins) are plentifully available from the body's normal breathing. According to the 1978 report on biotransmutation, excess energy is produced.

Geological Nuclear Reactions

The possibility that the Earth's internal heat is partially generated by nuclear reactions (that are not commonly understood radioactive decay) was, in part, the motivation that led physicist Steven E. Jones of Brigham Young University to his research in the period leading up to 1989. Jones and his group speculated that elevated helium-3 and tritium in volcanic emissions might indicate a geological cold fusion mechanism at work.[48] These functions might even be occurring in the core of the giant outer planet Jupiter, the group suggested, which would explain the excess radiated heat of that planet (about twice the solar radiation input). The group suggested that cold fusion in terrestrial matter might even be responsible for localized volcanism. The Jones group believed that the presence of localized concentrations of He-3 in diamonds and in metal foils was evidence of "cold piezonuclear fusion." Though he does not accept the nuclear basis of excess heat in "cold fusion" experiments, Jones still takes very seriously the geological/astrophysical implications of his work. (See his Letter to the Editor in this issue of *Infinite Energy*, p. 8.)

Theoretical Models

Biotransmutations are well discussed by Mizuno[22] and by Kozima.[5] An all embracing theory must account for the long (500 hours) delay in a switch on of nuclear effects in electrolysis of D-Pd-LiOD, irreproducibility that is diminished by the use of codeposition of D_2 and Pd on Au, and by use of thin films; internal damage in Pd; burst-like nature of the phenomena in Pd; occurrence in organic and biomaterials. No theory yet published accounts for all these effects. Mizuno[22] supports the ideas of E. Conte in which neutrons are formed by proton-electron combination. Kozima[5] has made a detailed case in which neutrons in the ambient enter all substances and are trapped there. These entities then undergo various reactions with Li, H, D, T, etc., and produce the observed effects. Although Kozima's model serves to explain the widespread nature of the effects (he sees neutrons as being "in" everything), his numerical analyses show the required neutron concentration to vary from 10^4 to 10^{13} cc^{-1} in D-Pd to achieve consistence, with results and this seems too large a range for consistency. Stress is put in Kozima's model on reaction with Li^6 but it was shown in 1990 by Appleby and Srinivasan[29] at Texas A&M that there was no difference in the heat evolved in Pd-LiOD in systems containing only Li^6 and those containing natural Li.

There are now literally dozens of other theories purporting to explain low-energy nuclear reaction phenomena. It will be a long time before all these are sorted out into viable and nonviable. For the present, work continues to determine just how big is the "iceberg" of the low-energy nuclear reaction phenomenon, popularly thought to have begun with the work of Fleischmann and Pons. More grist for the theorists' mill.

Conclusions

Evidence (Table 2) for nuclear reactions in the cold comes from systems involving H and D in metals: tritium, helium, and new metals are produced. Arcs through gases and liquids give products containing new nuclei. Application of heat and electrolysis gives nuclear change in general. The latter also purportedly occurs when trash is heated anaerobically. Finally, there is strong evidence for nuclear reactions in biological systems. It is speculatively suggested that scientifically verified cases in which people remain well for years without imbibing more than a communion wafer per day while doing manual labor might sustain themselves by means of nuclear reactions in the body. Thus, in 1999 it is known that cold nuclear reactions occur in metal-H—radioactive, chemical, and biological systems. May they, then, be widespread through Nature, such as in geological and many astrophysical systems?

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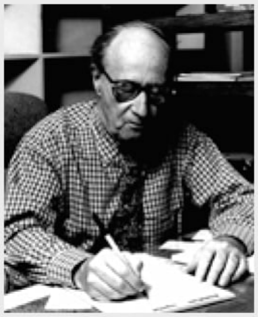
About the Authors



Dr. John O'M. Bockris received a B.Sc. degree in Chemistry with First Class Honors from the University of London in 1943, the degree of Ph.D. in 1945, and the degree of D.Sc. in 1952. After receiving his Ph.D., he became a member of the faculty of Imperial College, where he formed a significant research group in electrochemistry. After emigrating to the United States, he became Professor of Chemistry at the University of Pennsylvania in 1954. From 1982-1997, Bockris was a Distinguished Professor of Chemistry at Texas A&M University. He is the author of two books and numerous articles. Bockris founded the International Society for Electrochemistry (1949) and helped found the International Society for Hydrogen Energy (1974). He has received numerous awards and appointments in his field.

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Wisdom is not how much you know, but how you use what you know.

Submitted by Walter S. Rosenthal

Nuclear Transmutation Reaction Caused by Light Water Electrolysis on Tungsten Cathode Under Incandescent Conditions

T. Ohmori[*] and T. Mizuno[**]

Abstract

When a tungsten cathode is electrolyzed at high power, it exhibits an intense reddish-purple glow discharge, and emits radio frequency (RF) electromagnetic waves. In some cases powerful excess heat, ranging from 60 to 140 watts is generated, and substantial amounts of new elements are formed, including Fe, Cr, Ti, Ca, Ni, C, Re, and Pb. This has been observed with many different electrolyte solutions including Na_2SO_4, Na_2CO_3, $NaClO_4$, K_2CO_3, KNO_3, Rb_2CO_3, Cs_2CO_3, Ba $(NO_3)_2$ and $Ba(ClO_4)_2$. Based on the heat and apparent transmutations, we conclude that some form of nuclear reaction occurs. We propose several different nuclear reactions that might plausibly account for the phenomenon.

Introduction

In recent years, experimental results achieved by a number of independent researchers support the finding that nuclear reactions can occur under room temperature conditions in metal hydrides, but these reactions are not necessarily conventional deuteron-deuteron fusion reactions. They may be fission or fusion reactions involving metallic elements in the electrode, and not just hydrogen or deuterium.[1-17] In an Au/H_2O electrolysis system, considerable amounts of Hg, Kr, Ni, Fe and, in some cases, Si and Mg were produced on and in the electrode, apparently as a result of the nuclear fusion or fission reactions involving the Au electrode material.[7-8] This suggests that the excess heat reaction might be enhanced by employing as electrode material a metal with a large atomic number, since it will have relatively small nuclear binding energy. In this respect tungsten (W) would be one of the most favorable electrode materials because it has a large atomic number and resistance to high heat. For this reason, we selected W as the working electrode material.

In the present study, strong excess energy generation, new element production, and two other noteworthy phenomena—glow discharge and RF electromagnetic wave emissions—were observed. We report these results and propose some nuclear transmutation reactions that might explain the phenomenon.

Experimental

The electrolytic cell is made of quartz glass. It is simpler than the vessels used in previous experiments, with a design intended to facilitate excess energy measurements (Figure 1). The total volume is 240 ml. The working electrode is a rectangular W foil (0.5 cm^2 nominal area, 0.1 mm thick). The surface is scraped with a glass shard which has been cleaned in warm aqua regia and washed with pure water. A lead wire of W or Ni (0.4 mm diameter) is connected to the center of the rectangle. A W lead wire is used in experiments intended to quantify transmutation products. The counter electrode is a Pt mesh (1 cm x 7 cm, 80 mesh) connected with a Pt wire (0.4 mm diameter). The lead wires are covered with Teflon. Before the working electrode is installed in the electrolytic cell, the cell is cleaned in a hot acid solution (1:1 H_2SO_4 + HNO_3), then rinsed with Milli-Q water several times and placed in an ultrasonic cleaner in a bath of Milli-Q water.

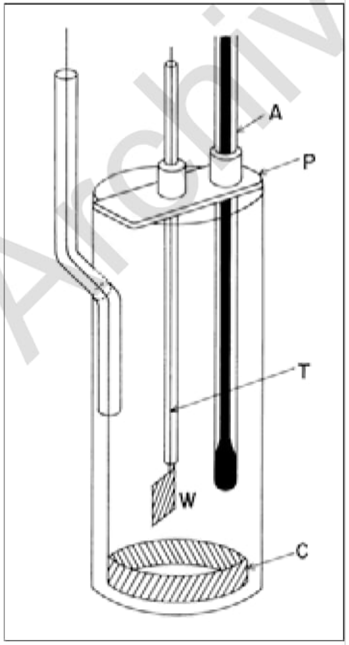

Figure 1. Electrolytic cell: (A)-alcohol thermometer, (C)-Pt counter electrode, (P)-plastic plate, (T)-Teflon tube, and (W)-working electrode.

Electrolysis was performed in 0.5 M Na_2SO_4, Na_2CO_3, K_2CO_3, $NaClO_4$, Ba $(ClO_4)_2$, and $Ba(NO_3)_2$, 0.25 M Rb_2CO_3 and Cs_2CO_3, and 0.16 M Cs_2CO_3 solutions prepared with Milli-Q water. The Na_2SO_4, Na_2CO_3, and K_2CO_3 reagents are Merck sprapur grade and other reagents are analytical pure grade. We also tested an alloy of 50% Mo - 50% W in electrolytes of 0.5 M K_2CO_3, H_2O, and W in 0.5 M K_2CO_3, D_2O. The volume of the electrolyte solutions ranged from 120 to 170 ml (usually 150 ml). Impurities in the 0.5 M Na_2SO_4 solution included: Ba (0.2-0.1 ppm), Si (0.1-0.01 ppm), K (0.01-0.001 ppm), Li, Mg, Ca, P, Sr, Cr, Fe, Co, Ni, Cu, Zn, As, Hg, Pb (0.001-0.0001 ppm), Ti, Mn, and Cd (< 0.0001 ppm). Impurities in the Pt mesh included: Rh (18 ppm), Pd, Cr, Si (2 ppm), Cu, Fe, B, and Cd (< 1 ppm). The amounts of Fe and Cr contained in the W electrode material are < 1 ppm. The electrolysis cells were placed in a constant temperature air chamber, thermostatically controlled at 20±1°C. A regulated DC current/voltage supply with 160V maximum output was used.

After electrolysis, transmuted elements on the surface and in the bulk of the electrode were identified and quantified with an energy dispersive X-ray analyzer (EDX), electron probe micro analyzer (EPMA), and secondary ion mass spectrometer (SIMS). The isotopic distribution of the product elements was determined by SIMS.

Results and Discussion

Incandescence of the Electrode and Glow Discharge

Under electrolysis with the current below 2.5A, the working electrode did not incandesce, and the only chemical reaction to occur was H_2 evolution. However, at current densities above this, the electrode began to incandesce and emit an intense reddish-purple glow (See *Infinite Energy*, No. 20, Figure 1, p. 20). The input voltage jumped from ~20V to 160V and the current fell to 1~0.8A and then decreased gradually down to 0.7~0.5A. When the glow discharge began, strong RF electromagnetic emissions began, which disrupted the thermocouple, so the solution temperature was measured with an alcohol thermometer (Figure 1). We assume the glow is caused by the plasma, and the particle acceleration within it induces the strong RF noise. The incandescent state of the electrode continued as long as the electrolysis conditions were maintained.

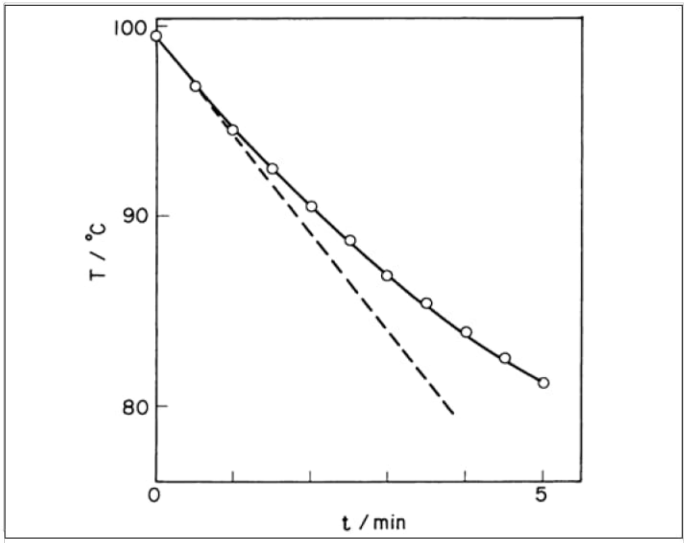

Figure 2. The cooling curve of the electrolyte solution after it reached boiling and power was turned off. The solution nearly filled the cell.

Excess Energy Generation

The temperature of the solution just before the initiation of incandescence was 80 to 85°C. After initiation, the temperature increased sharply and reached the boiling point within 80 seconds. Excess energy measurement was made mostly for 15 to 30 minute periods under incandescent conditions, using the following equation:

$$E = W_{\mathrm{vap}} + W_{\mathrm{sol}} + W_{\mathrm{cell}} + W_{\mathrm{wall}} - W_{\mathrm{in}} \qquad (1)$$

where: W_{vap}: heat required to vaporize light or heavy water
W_{sol}: heat required to raise the temperature of the solution just before the incandescence of the electrode up to the boiling point
W_{cell}: heat required to raise the temperature of the container of the cell
W_{wall}: heat effused through the wall of the cell during the measurement
W_{in}: electric power applied.

The W_{wall} was determined from the time derivative of the temperature, $(\delta T_s/\delta t)_{t=0}$, at a starting point (*ca.* 99°C) of a natural cooling curve (Figure 2) of a boiling electrolyte solution which nearly fills the electrolytic cell. This is represented by the following equation,

$$\mathrm{W_{wall}} = \mathrm{C_sM_s}(\delta T_s/\delta t)_{t=0}\, t_{\mathrm{m}} + \mathrm{C_qM_c}(\delta T_s/\delta t)_{t=0}\, t_{\mathrm{m}} \qquad (2)$$

where: $\mathrm{C_s}$ and $\mathrm{C_q}$: specific heats of the electrolyte solution and quartz of cell material
$\mathrm{M_s}$ and $\mathrm{M_c}$: masses of the electrolyte solution and the cell
t_{m}: measurement time.

The excess energies obtained in thirty-one electrolysis systems are listed in Table 1, which shows that large amounts of excess heat were generated in every test, the yield being virtually the

Table 1. Excess Energies and Energy Efficiencies of Various Electrolysis Systems

Solution	Volume (cc)	Time (min)	Current (A)	W_{vap}	W_{sol}	W_{wall} (KJ)	W_{cell}	W_{tot}	W_{in}	Excess Energy (W)	Efficiency (%)
W-Electrode											
0.5M K_2CO_3	170	30	0.48	133	18	179	4	334	138	108	242
K_2CO_3	170	30	0.54	159	18	179	4	360	155	114	232
K_2CO_3	150	30	0.53	125	13	179	4	321	153	94	210
K_2CO_3	150	30	0.53	86	18	179	4	287	153	75	188
K_2CO_3	120	15	0.65	87	9	90	2	188	94	105	200
K_2CO_3	120	15	0.60	64	10	90	2	166	86	88	193
K_2CO_3 (*)	150	30	0.56	133	11	179	4	327	160	92	204
K_2CO_3 (*)	150	30	0.43	49	13	179	4	245	124	65	198
K_2CO_3 (**)	150	15	0.73	93	8	90	2	193	105	97	183
Na_2CO_3	150	30	0.63	99	14	179	4	296	179	65	165
Na_2SO_4	170	30	0.67	193	16	179	4	392	193	111	203
Na_2SO_4	170	30	0.70	161	17	179	4	361	201	89	180
Na_2SO_4	170	30	0.65	156	14	179	3	352	187	92	188
Na_2SO_4	120	15	0.55	67	12	90	4	173	79	66	165
$NaClO_4$	140	15	0.70	85	5	90	3	183	101	92	181
$NaNO_3$	150	6	0.63	36	13	37	4	90	37	141	243
$NaNO_3$	150	23	0.63	110	14	135	4	263	137	93	192
KNO_3	150	30	0.58	144	13	179	4	340	167	96	204
KNO_3	150	30	0.64	139	13	179	4	335	184	83	182
0.25M Rb_2CO_3	170	30	0.59	218	18	179	4	419	170	138	246
Rb_2CO_3	120	15	0,78	81	11	90	3	185	112	80	165
Rb_2CO_3	150	30	0.58	102	15	179	4	300	167	74	180
Rb_2CO_3	150	30	0.43	70	14	179	4	267	124	78	215
0.16M Cs_2CO_3	170	30	0.47	95	12	179	3	289	135	85	214
0.25M Cs_2CO_3	150	15	0.59	84	14	90	4	192	85	119	226
Cs_2CO_3	150	30	0.44	86	17	179	4	286	128	88	223
Cs_2CO_3	150	30	0.62	100	17	179	4	300	179	68	168
0.25M $Ba(ClO_4)_2$	170	30	0.49	121	16	179	4	320	141	99	226
$Ba(ClO_4)_2$	170	15	0.52	74	11	90	3	178	75	114	237
$BaNO_3$	150	30	0.44	73	16	179	4	272	127	81	214
W50%-Mo50% Electrode											
0.5M K_2CO_3	150	30	0.59	133	14	179	4	330	170	89	194

(*) W wire electrode (**) in D_2O solution

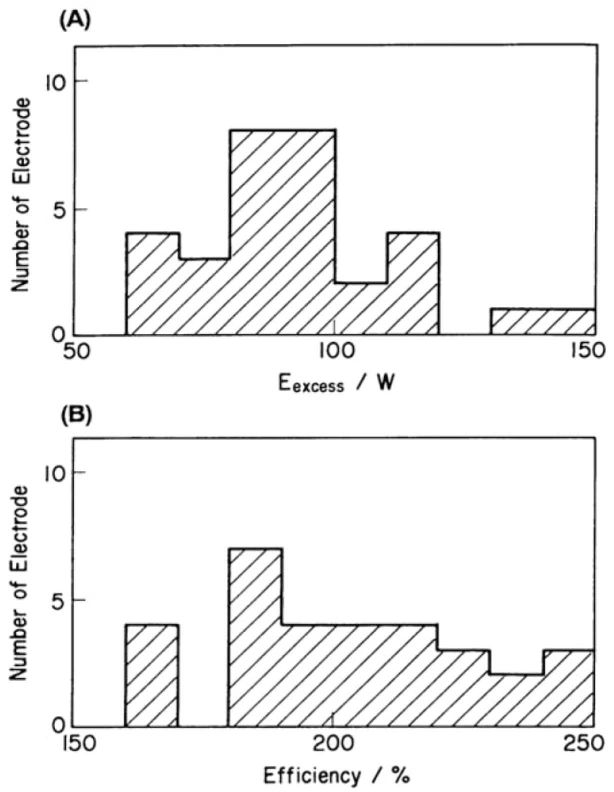

Figure 3. Distribution of excess energy and efficiency in the Au / H_2O electrolysis systems; (A) excess energy, (B) efficiency.

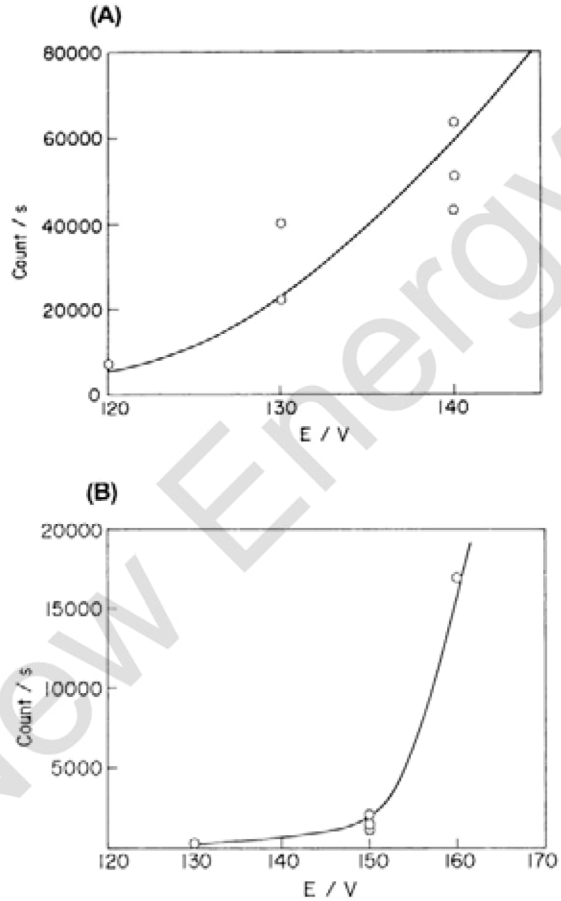

Figure 4. Intensities of the emission from the incandescent electrode counted by the REM neutron counter; (A) in Na_2SO_4, and (B) in K_2CO_3.

same whatever electrolyte was used. Apparently, alkali metal and alkali-earth metal cations and carbonate, sulfate and perchlorate anions play little or no role in any of the nuclear reactions generating excess energy. Figure 3 shows the distributions of the excess energies and the energy efficiencies obtained in all of the electrolysis systems listed in Table 1. Excess power was between 60 and 140 watts, centered mainly in the range 80 to 100 watts. Energy efficiency, output as a percent of input, was 150 to 220%, mainly in the range of 180 to 200%.

Electromagnetic Wave and Neutron Measurement

Under incandescent conditions at the electrode, strong emissions were detected by the neutron counter. However, the REM neutron counter used in this study detects electromagnetic waves as well as neutron activity, and it cannot distinguish between them. Judging by the fact that the electrical noise was so intense it interfered with the functioning of the thermocouple, we conclude that most of the signal detected by the neutron counter must have been caused by electromagnetic emissions rather than neutrons, although there is no denying the possibility that neutrons may also have been detected.

Figure 4 shows the intensity of the emissions plotted against input voltage in 0.5 M Na_2SO_4 and 0.5 M K_2CO_3 solutions. The emission intensity tends to increase nearly exponentially with voltage, depending on the nature of the electrolyte. This suggests that the emission is not induced from the electric power supplies or circuits but from the inner part of the cell, being caused by some form of charged particles (plasma) produced by the glow discharge from the incandescent electrode.

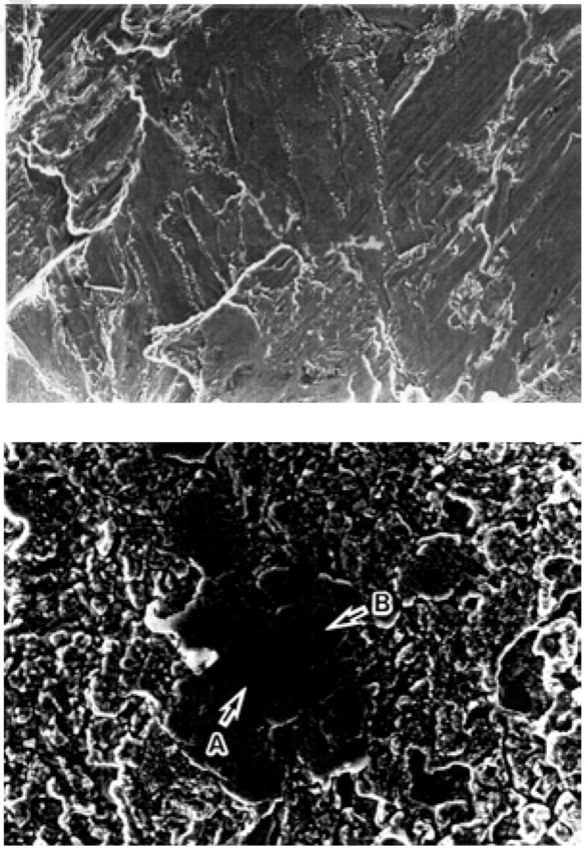

Figure 5. SEM images of the W-Type I electrode surfaces before and after the electrolysis; (1) before electrolysis, (2) after electrolysis. The duration of the glow discharge of the electrode is ten minutes. Arrows A and B in the image (2) show the measurement spots of EDX spectra in Figure 8.

Transmuted Element Production

After glow discharge electrolysis, remarkable anomalous structures were found on the electrode surfaces. Figure 5 shows scanning electron microscope (SEM) images of the electrode before and after glow discharge electrolysis. As seen here, the whole surface of the latter electrode is covered with a rugged structure not unlike that deposited by a volcanic lava flow. The W layers near the electrode surface have melted. This indicates the temperature may have risen to 3400°C or more. In addition, one can see a crater-like structure at the center of the melted area. Considerable amounts of several unexpected elements were detected in this area. The EDX spectra of freshly prepared electrode surfaces before electrolysis are shown in Figure 6a. Typical EDX spectra of the electrode surface after glow emission in 0.5 M Na_2SO_4 solution are shown in Figure 6b, with pronounced Fe, Cr, and Ni signals. Cathodes producing this spectra will be referred to hereafter as W-Type I. A few cathodes produced spectra like those shown in Figure 7, with strong Fe, Ti, and Ca signals. These will be referred to as W-Type II. The signal strengths of both Fe and Cr in Figure 6 (W-Type I) and Fe and Ca in Figure 7 (W-Type II) are comparable to those of original W electrode material before electrolysis. As described above, the amounts of Fe, Cr, and Ti found as impurities in the cell materials prior to electrolysis are infinitesimally small. The amount of Ca should not be large enough to give such a strong EDX signal even if all of the impurities in the entire cell were deposited on the electrode. In addition, the fact that two completely different types of EDX spectra (*i.e.* W-Type I and W-Type II) were obtained in the same kind of electrolysis system supports the hypothesis that these elements are not impurities but reaction products.

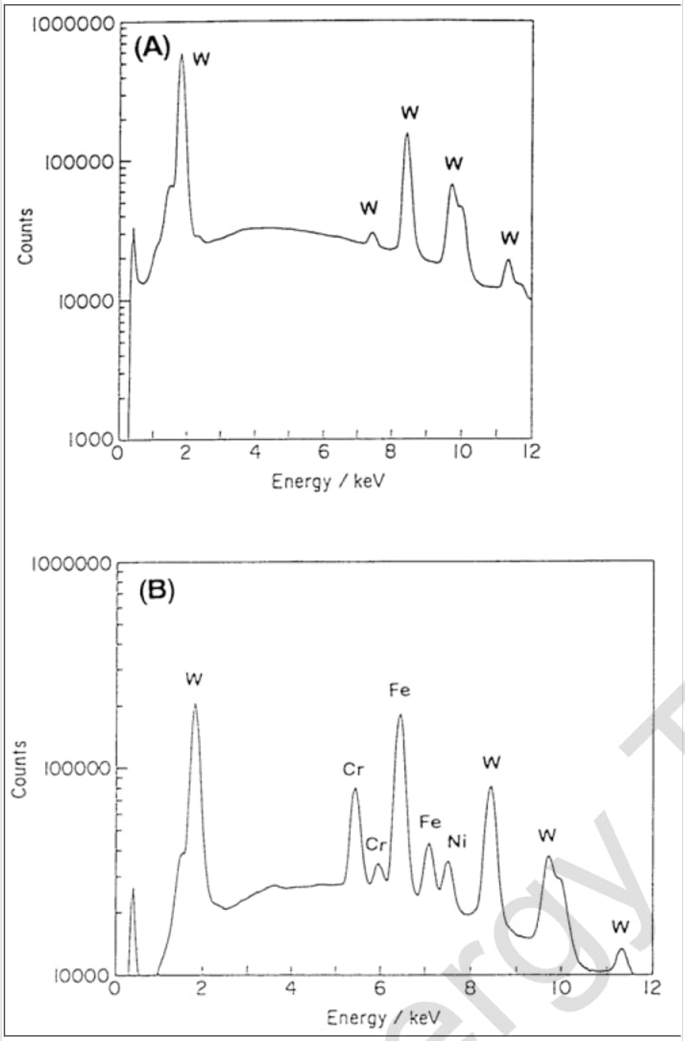

Figure 6. Typical EDX spectra in the W-Type I electrodes before and after the electrolysis; (A) before the electrolysis, (B) after the electrolysis.

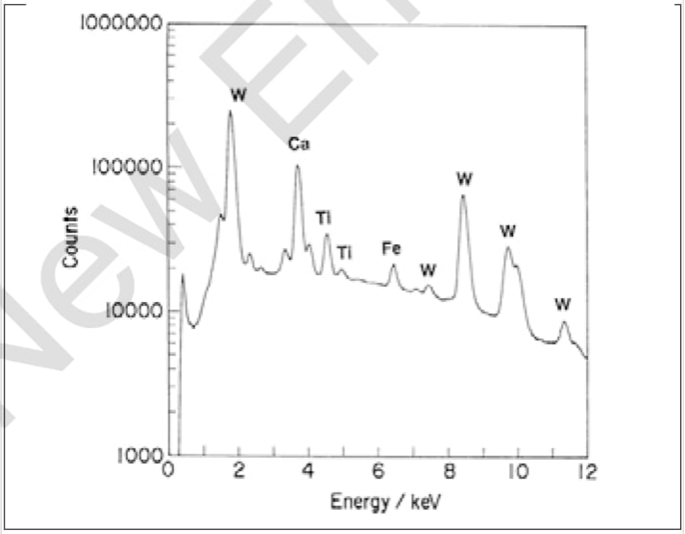

Figure 7. Typical EDX spectra on/in the W-Type II electrodes after the electrolysis.

Figure 8 shows the EDX spectra observed at two different spots on the crater-like structure shown in Figure 5 (which is W-Type I); one is a spot at the center part (point A) and the other is a spot near the edge (point B). In the spectrum from point A, strong signals of Fe and Cr and a moderate signal of Ni are observed, from which the contents of Fe, Cr, and Ni are estimated at 67.4, 16.9 and 7.9 at.% (atomic percent), respectively. The content of W is only 7.8 at.% in that location. However, the contents of Fe, Cr, and Ni decrease markedly with increasing

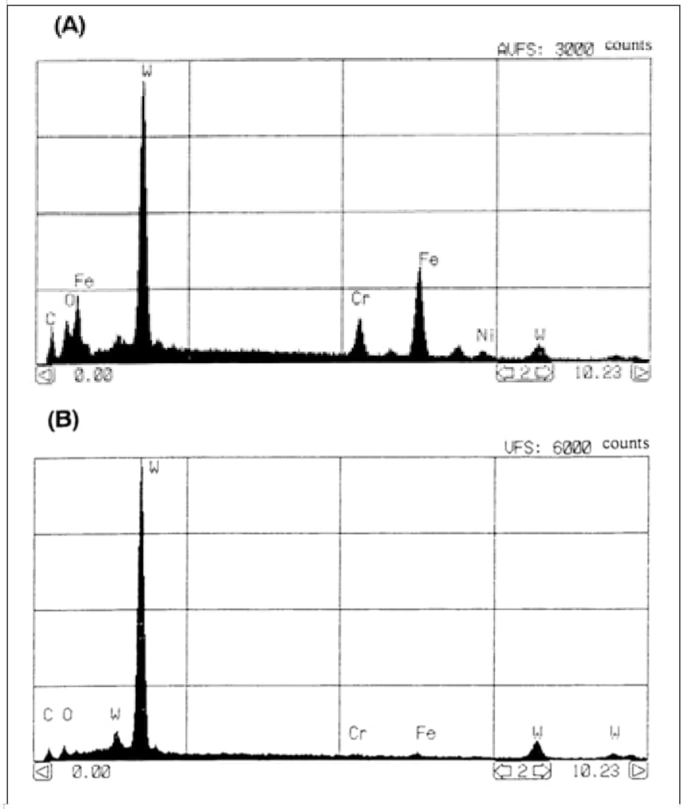

Figure 8. EDX spectra at points A and B of the crater-like structure shown in Figure 5.

distance from the center of the crater. As a result, Fe, Cr, and Ni at point B are reduced to 6.7, 1.9 and 0 at.%, respectively. This result is also obtained with EPMA analysis.

Isotopic Distribution

Besides Fe, Cr, Ni, and C, small amounts of Pb (0.6 at. %) and Re (0.3 at.%) were detected in the outermost layers of the W-Type I electrode to a depth of 160 Å. The isotopic distributions of Fe, Cr, Ni, Re, Pb, and W present in the outermost layers and in the bulk layers ranging between 1760 and 2400 Åare shown in Table 2 together with their natural isotopic abundance. In this calculation, the isotopic contents of ^{54}Cr, ^{54}Fe, ^{61}Ni, ^{62}Ni, and ^{64}Ni are assumed to be equal to their natural isotopic abundance for convenience because there is a suspicion that their SIMS signals are overlapping with those from other metal oxide ions. Consequently, the isotopic distributions for Cr, Fe, and Ni, exhibited in Table 2, is not strictly accurate.

For W, the ratio of the lightest isotope, ^{182}W, is slightly lower than its natural value at the outermost layers of the electrode, but elsewhere the isotopic distribution is normal. For Cr and Ni, slight deviations from the natural isotopic abundance are observed. For Fe, the deviation is very small. In contrast, for Pb and Re, the deviation is notable. Although the isotopic deviation for Fe scarcely occurred in the W/H_2O electrolysis systems, it was quite pronounced in the Au/H_2O, [1,3,5-8] Pd/H_2O, [18] and Pd/D_2O [2,4] electrolysis systems. Similar isotopic deviation was observed for Fe detected in the electrodes after the electrolysis in Au/H_2O [13] and Ni/H_2O [14] electrolysis systems.

In view of the overlapping of Fe, Cr, and C at a center part of the crater-like structure of the W-Type I electrode surface there would be little doubt that the ^{56}Fe, ^{52}Cr, and ^{12}C atoms were produced simultaneously in that location by some sort of nuclear reaction. These reactions would be responsible for the generation of enough excess energy to push the W electrode to emit the plasma glow and the electromagnetic waves at input power levels too low for this to happen normally.

Table 2. Isotopic Content of New Elements Produced on/in the Electrode and Tungsten of the Electrode Material

Element	Isotope	Isotopic content within 160 Å from the electrode surface (1st run) (at.%)	Isotopic content within 1760-2400 Å from the electrode surface (11-15th runs) (at.%)	Natural isotopic abundance (at.%)
Cr	^{50}Cr	6.1		4.31
	^{52}Cr	79.1		83.75
	^{53}Cr	12.4		9.55
Fe	^{56}Fe	91.0		91.66
	^{57}Fe	2.9		2.19
Ni	^{58}Ni	63.0		67.98
	^{60}Ni	30.8		26.23
Re	^{183}Re	48.4		37.1
	^{185}Re	51.6		62.9
Pb	^{206}Pb	38.5		25
	^{207}Pb	55.0		22
	^{208}Pb	6.5		52
W	^{182}W	25.3	$26.6^{+0.6}_{-0.5}$	26.3
	^{183}W	14.9	$14.5^{+0.3}_{-0.4}$	14.3
	^{184}W	30.4	$30.5^{+0.6}_{-0.5}$	30.6
	^{186}W	29.0	$28.2^{+0.8}_{-1.2}$	28.6

Plausible Nuclear Reactions

The results obtained in the present study, *i.e.* excess energy generation, glow and electromagnetic wave emission, transmuted element production and the isotopic deviation observed in some of these transmuted elements, forces the conclusion that some sort of nuclear reaction must have occurred during electrolysis. From the SIMS analysis, the volumes of Na, B, Mg, and Li at the outermost layers of the electrode were found to be 1.9, 1.18, 0.06, and 0.0008 at.%, which was far less than the levels for Fe or Cr. The isotopic distributions of B and Mg are not much different from their natural isotopic abundance. In addition, Li and Mg were not detected in the electrolyte solution after electrolysis by the ICP analysis. This fact suggests that the major nuclear reactions are not induced by nuclear fusion reactions involving only deuterons and protons. If the new elements, *e.g.* Fe, Cr, etc., are produced as a result of the combinations of some nuclear fusion reactions starting from deuteron-deuteron or deuteron-proton reaction, fairly large amounts of new elements lighter than Cr, at least comparable to the discovered quantities of Fe or Cr, should be detectable in the W electrode or in the solution after electrolysis.

It is well known that a very strong electric field exceeding 108 V/cm is formed across the electric double-layer at the electrode/solution interface. Under such strong electric fields, electrons passing through the double-layer by tunneling may, perchance, be captured by protons or H atoms as follows,

$$H^+ + e^- \rightarrow \text{neutron} \tag{A}$$

although an overwhelming majority of the electrons are consumed by the hydrogen evolution reaction. With an increase in the electric field the probability of the reaction (A) would be enhanced rapidly, which might make the neutron emission possible (Figure 4). The reaction (A) is endothermic so that the neutrons produced should be thermo-neutrons with a low kinetic energy. Consequently, the greater part of the neutrons thus produced would be captured by the W electrode material, for example in the following reactions:

$$^{182}W + n\ \text{neutron} \rightarrow {}^{182+n}W \tag{B}$$

$$^{182}W + \text{neutron} \rightarrow {}^{183}Re \tag{C}$$

$$^{184}W + \text{neutron} \rightarrow {}^{185}Re \tag{D}$$

A slight decrease in the content of ^{182}W on the outermost layers of the electrode (Table 2) supports the possibility of these reactions. The $^{182+n}$W produced should be unstable, disintegrating to form Fe, Cr, and C, for example, for W-Type I electrodes as

$$^{184}W \rightarrow {}^{56}Fe + 2\,{}^{52}Cr + 2\,{}^{12}C + 12\beta^- \tag{E}$$

$$^{184}W \rightarrow 2\,{}^{56}Fe + {}^{52}Cr + 5\,{}^{4}He + 12\beta^- \tag{F}$$

$$^{184}W \rightarrow 3\,{}^{56}Fe + 4\,{}^{4}He + 12\beta^- \tag{G}$$

or likewise to form Ti and Ca, for example, for W-Type II electrodes as

$$^{184}W \rightarrow 2\,^{48}Ti + 2\,^{40}Ca + 2\,^{4}He + 14\beta^- \qquad (H)$$
$$^{184}W \rightarrow 2\,^{56}Fe + {}^{40}Ca + 8\,^{4}He + 14\beta^- \qquad (I)$$

Perhaps this is why the production of Fe, Cr, and C were closely linked. These nuclear transmutation reactions are assumed to occur locally in the W electrode. This would promote the development of the crater-like structure seen on a W-Type I electrode or the fine crater structure formed on an Au electrode in the Au/H_2O electrolysis system.[3,7,8] Perhaps these reactions occur at the rim of micro-cleavages and edges of the fractured face of the electrode, since it was confirmed that the craters are developed mainly at those areas. Tunneled electrons and protons and/or H atoms may become highly concentrated at the cleavages and edges of the fractured face, forming an anomalous phase. We suspect that the probability of reaction (A) is enhanced when this occurs. Recently, in this context, the idea that the formation of electron clusters on the electrode surface promotes the nuclear transmutation reaction was proposed by Hal Fox.[19] The nuclear transmutation reactions, (B) to (I) would release γ-rays, the energy of which would be expended on the relaxation by the surrounding W atoms of the electrode. As a result of this, X-rays would be emitted from the electrode, as have been detected by some researchers in Ni/Li_2SO_4, H_2O [14] and in Pd/LiOD, D_2O electrolysis systems.[17-20] In the present study we observed that the clear reddish-purple glow extended into the bulk of the solution, completely enveloping the incandescent electrode. This means that the generation of the plasma occurs even in solution separated by the distance of *ca.* 2 cm or so from the electrode.

Thus, strong evidence of low temperature nuclear transmutation reactions was obtained. However, the mechanism appears to be very complicated. The reaction schemes we propose here are not conclusive and may only be a few of the many nuclear reactions that might occur simultaneously in this electrolysis system. In addition, it remains unclear why two types of W electrodes exist, showing different spectra, presumably caused by different reaction schemes. To clarify these issues, more detailed investigations in various fields will be necessary, including spectroscopic analysis of the glow and electromagnetic wave, clarification of the product elements and their distribution in the electrode, clarification of the product elements in solution, detection of X-ray, γ-ray and neutron emissions and their characterization, morphology of the electrode surface, etc.

Nevertheless, we believe that it would not be so difficult to scale-up in output power levels of 1,000 to 10,000 kW by utilizing the low temperature nuclear transmutation reactions occurring in the W/H_2O electrolysis system, judging from the fact that the excess power of 200 watts was generated from a W electrode of only 0.5 cm^2.

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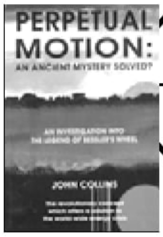

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DEVICE AND PROCESS TESTING UPDATES

Jeffery D. Kooistra and Ed Wall

NERL has been a busy place the last few months. With the arrival of Jeffery D. Kooistra as research scientist, new projects have been started and old matters have been moved from back burner to front. Even before Jeff had completed his move to New Hampshire, Ed had been building Jeff an office. Once on site, Jeff and Ed finished the office and Jeff moved in.

Jeff brought along his laboratory from Michigan, so NERL is now a place where advanced Marinov Motor work is being done.

To make room, Jeff and Ed have been putting lots of time into clearing out and reorganizing the lab. There are several new workbenches and shelves in place, and the laboratory has become both more scientist friendly and visitor friendly as well.

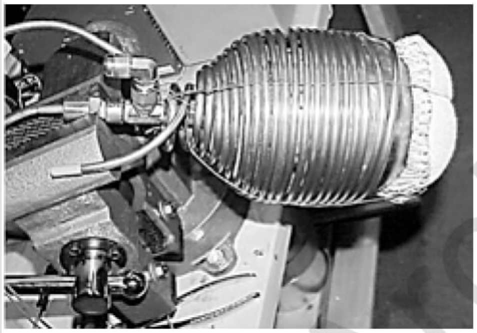
Along with new work on Marinov Motor related issues (most of which is currently proprietary), studies on railguns and retrograde railguns has commenced. For a slight investment, a great deal of interesting science can be done. The crackle and sizzle of sparks, and the scent of ozone in the air, is common when the railgun is being used. We've verified the expected "armature-toward-the-muzzle" behavior, and also demonstrated the retrograde motion of the ferromagnetic armature.

Ordinary railgun armatures do not move all that fast when a car battery is used as a power supply. However, we've already managed to up our performance level about an order of magnitude while using LESS energy. Exactly *how* we do this is, at the moment, also proprietary information. We're planning to do more precise measurements soon with yet another modification that should at least double current performance.

Case Cell

The flow calorimeter was used in a long series of measurements of cell heat output. This is the first time, to our knowledge, that flow calorimetry has been performed on this type of cell. Heat measurement before this has been performed by temperature measurement of the catalyst (isoperibolic method). This method has been cause for criticism, because it does not directly measure heat. It compares temperatures attained in the catalyst when ordinary hydrogen is in the cell to temperatures when deuterium is used, for the same heater power. We have found some problems with this method when thermal disequilibrium is introduced to the cell. We found substantial excess temperature in the cell for a wide range of heater power, even for temperatures below where the cell is supposed to "turn on." This was cause for initial excitement, which was quickly dulled when we realized that the flow calorimeter was not showing excess heat. We then reduced heater power to levels below the crossover of the calibration and deuterium lines and found that the deuterium data actually stayed on course, not returning to the calibration line. This was the final confirmation that the excess temperature, in this case, did not indicate excess heat. See Figures 1 and 2 and accompanying photograph.

The calorimeter performed well, but the calorimeter envelope had disappointing heat capture. This has been completely redesigned. The new setup has a much larger envelope that does not use a Dewar (modeled on Earthtech work) and we have included a cold trap for the vacuum pump. The purpose of the cold trap, which is an apparatus immersed in liquid nitrogen, is to force the gas that is between the vacuum pump and the cell to be exposed to extreme cold. This very effectively eliminates the backstreaming of pump oil, which Les Case has warned us may be a problem. It also keeps the substantial moisture and other detritus that is sucked from the cell from entering the pump. This will mean that the purging of the cells is more efficient, with much deeper vacuum, much less contamination in the cell, and longer pump oil life.



Flow calorimeter cooling coils around Case cell.

This new setup has been constructed with cooling coils around the top of the cell to allow for thermal disequilibrium. The degree of disequilibrium can be controlled by changing the inlet water temperature in the flow calorimeter and by adding to or reducing the number of coils wrapped around the cell.

Mizuno-Ohmori Cell

Jed Rothwell has been busy visiting Japan. He took the opportunity to see Drs. Mizuno and Ohmori and provided us and Scott Little of Earthtech with a wealth of photographs of virtually everything in Mizuno's laboratory. He expounded artfully on the method of calorimetry employed and is producing a detailed description to be published in the next issue of *Infinite Energy*.

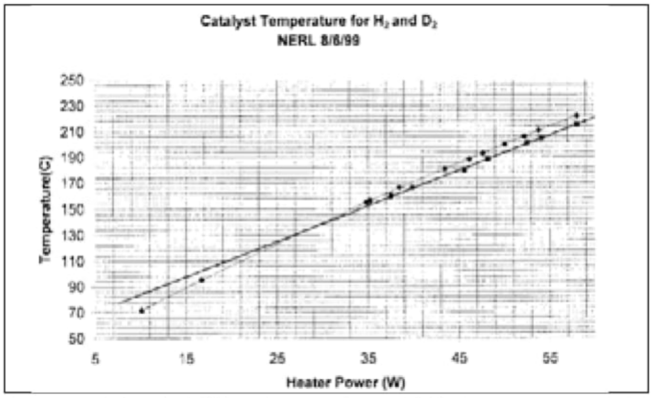


Figure 1. Thicker straight line is H_2 calibration. Thin line is D_2 thermocouple data.

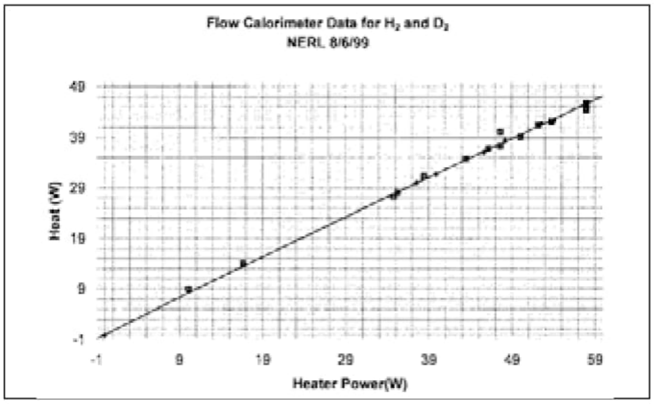


Figure 2. Line is H_2 calibration. Data points around it are from D_2 runs.

COMPILED BY EUGENE MALLOVE

Challenge to Very Good Science

Prof. George Miley's Historic U.S. DOE Low-Energy Nuclear Reactions Contract Attacked by Cold Fusion Critics

In the last issue of *Infinite Energy*, we reported the good news that Prof. George Miley's Low Energy Nuclear Reactions (LENR) Group at the University of Illinois had been awarded a U.S. Department of Energy contract—for an experimental study to verify previously tested electrolytic techniques to remediate radioactive nuclides. This was to be the first *overt* DOE cold fusion funding since 1989.

As predicted, the good news soured. Cold fusion critics, such as Dr. Robert L. Park of the American Physical Society, learned of the Miley award and apparently immediately went on the attack. Numerous contacts were made to DOE to attempt to derail the peer-reviewed award to Miley's research group. As a result, Miley's funding of approximately $100,000 has been held up by DOE, pending "review" of the science behind the award by an *un-named* (!) panel of three individuals. (Other universities winning these NER —Nuclear Energy Research Initiative—awards apparently have received their funding already.)

Science magazine (Vol. 285, 23 July 1999, pp. 505-506) ran a "News of the Week" story by David Malakoff about the challenge, "DOE to Review Nuclear Grant." Malakoff commented editorially, "The restudy represents a potentially embarrassing stumble for DOE's new $19 million Nuclear Energy Research Initiative (NERI), which DOE officials pledged would use top-notch external reviewers to pick the best projects (*Science*, 11 December 1998, p.1980)...The project's apparent similarity to controversial cold fusion experiments—which have unsuccessfully sought to use electrochemical reactions to spark energy-producing nuclear fusion at room temperature—raised eyebrows both within and outside DOE."

It is known from both the *Science* article and discussions with Prof. Miley that NERI manager John Herczeg and DOE nuclear office head Bill Magwood have asked the DOE's Office of Science to review the matter—after reportedly proposing to Miley an unorthodox method of dealing with the matter that might have gotten bureaucrats Herczeg and Magwood "off the hook." One DOE engineer who knows the situation, but who is supportive of Miley's work, has let it be known that any blocking of the Miley funding may well be technically *illegal*. Illegal or not, the actions against Miley are repugnant and disgraceful, part of a pattern of unscientific action by U.S. Government officials within and associated with DOE. It all traces back to the 1989 Huizenga panel's rush-to-judgment report on cold fusion.

According to the *Science* report, Edwin Lyman from the non-profit Washington, DC arms control group, the Nuclear Control Institute, wrote to Energy Secretary Bill Richardson, "The credibility of DOE will be irreparably damaged unless funding for this cold fusion proposal is immediately withdrawn." Lyman probably knows nothing about cold fusion other than what he gathers from the likes of Robert Park's "What's New" column — thus is the pack mentality of the establishment propagated. Reporters like Malakoff never seem to question the basis for the opinions expressed by Lyman *et al.*, nor do they seek supportive comments by cold fusion scientists other than the hapless victim—in this case Miley.

Infinite Energy's Eugene Mallove maintains regular contact with U.S. Senator Bob Smith's (R-NH) office, keeping Senator smith and his advisors apprised of the current state of the cold fusion field. On this serious matter Senator Smith obliged with a thoughtful letter to the DOE, advising them to not be "closed to new ideas and approaches." At least *someone* in Washington can do the right thing! (Senator Smith, who formerly sought the GOP's Presidential nomination in the 2000 election, recently resigned from the Republican Party and briefly was slated to be the Presidential candidate for the conservative U.S. Taxpayer's Party.) The text of Smith's letter to DOE in full:

July 28, 1999

Mr. John C. Angell
Assistant Secretary for Congressional Affairs
U.S. Department of Energy
Forrestall Building, Room 7B138
1000 Independence Avenue
SW Washington, DC 20585-0800

Dear Assistant Secretary Angell:

My office was recently contacted by Dr. Eugene Mallove, Editor-in-Chief, *Infinite Energy* Magazine and Director of New Energy Research Laboratory located in Bow, New Hampshire. He is extremely concerned about an article in the July issue of *Science* Magazine which states that the Department of Energy is taking extraordinary measures to review a grant made to George Miley, a nuclear engineer at the University of Illinois.

It is my understanding that George Miley's academic credentials are well regarded and that his proposal was already subjected to peer review. What seems to have precipitated this unprecedented additional review is that his proposal is similar to work being done by scientists who are investigating "cold fusion." I am aware that there are some experts who view "cold fusion" as pseudo science and that DOE is concerned that its Nuclear Energy Research Initiative dollars are spent wisely. But to not investigate a potential method of transforming radioactive waste into harmless by-products, seems extremely short-sighted and perhaps the outcome of inherent bias at DOE toward conventional fusion technology.

I would encourage DOE to reconsider its placing of additional impediments in the way of this research project. Miley's research might be of great benefit to the problem of nuclear waste disposal, or it might prove that results seen in similar experiments were merely from accidental contamination. Either way, the research he proposes could be valuable. This grant represents an opportunity for DOE to show it is not closed to new ideas and approaches.

Please keep me apprised of your agency's decision on this matter. I have directed my Projects Director, Mark Aldrich, to be the point of contact. He can be reached in my Manchester office at 603-634-5000.

U.S. Senator Bob Smith

Sincerely,
Bob Smith, U.S.S.

October Cold Fusion Session at Prestigious Meeting

The 1999 Pacific Conference on Chemistry and Spectroscopy and the 35th ACS (American Chemical Society) Western Regional Meeting will be held October 6-8, 1999 at the Ontario Convention Center in Ontario, California. On Friday, October 8 there will be a full day session devoted to electrochemistry, with more than a half-day's worth of low-energy nuclear reactions talks. The program for the session, organized by cold fusion researcher Dr. Melvin Miles of the Navy's NASWC, China Lake, California, is as follows:

Morning Program:
Electrochemistry
8:30 Cold Fusion: Past, Present, and Future (M. Fleischmann)
9:20 The Present Status of Chemically Assisted Nuclear Reactions (E.K. Storms)
9:50 Anomalous Heat Production from Hydrogen Saturated Palladium (M.McKubre, F. Tanzella and P. Tripodi)
10:30 Thermal and Nuclear Events in a Polarized Pd+D System: An Overview of the Co-Deposition Method (S. Szpak and P.A. Mosier-Boss)
11:00 Electrolysis of Heavy Water with Titanium Cathodes: Heat Produced and Chemical Analyses of the Cathodes (J. Warner and J. Dash)
11:30 Theoretical Considerations for "Cold Fusion" Based Upon Co-production of Heat and Tritium (R.T. Bush and M. R. McGovern)

Afternoon Program:
Electrochemistry, Batteries, Fuel Cells
1:30 Calorimetric Studies of $Pd/D_2O+LiOD/Pt$ Electrolysis Cells (M.H. Miles)
2:00 The Impact of SEI Films Upon the Low Temperature Performance of Lithium-Ion Cells (M.C. Smart, B.V. Ratnakumar and S. Surampudi)
2:30 Development of Low Cost, Low Crossover Polymer Electrolyte Membranes for Direct Methanol Fuel Cells (G.K.S. Prakash, A. Atti, G.A. Olah, S.R. Narayanan, M.C. Smart, T. Valdez, S.Surampudi and V. Pleynet)
3:10 Factors Affecting the Performance of Direct Methanol Fuel Cells (T.I. Valdez and S.R. Narayanan)
3:40 Improved Thermal Batteries Using Molten Nitrate Electrolytes (M.H. Miles)

For further information about the entire conference or about this session, please visit http://www.vidrine.com/paccon.

Dynamic Ex-Computer CEO Notes Mallove's Work in Cold Fusion

Report Submitted by Sean Cavanaugh

I recently (July 1999) was invited to the launching of Intend Change, a multi-million dollar internet business launch held at the San Francisco Museum of Modern Art. During the presentation, Joe Firmage, the principal of Intend Change and former CEO of USWeb/CKS, one of the largest Internet firms to date, mentioned that he was now focusing on New Sciences and New Physics. Firmage was the founder and CEO of Serius Corporation and VP of Strategic planning at Novell. During the question and answer period, I stood up and asked in front of 300+ executives and press whether Mr. Firmage had heard of Dr. Mallove and his work in the field of LENR. Mr. Firmage acknowledged that indeed he had, that he had great respect for Dr. Mallove, and that he wanted to know more about the nuclear remediation work that he had heard about.

In August I received an e-mail from Joe Firmage stating he would be attending Dr. Mallove's San Francisco Investors Conference (SFIC) on August 25-26 in Marin County at the Waterfront Conference Center. Info on this event can be found on http://newenergy.hypermart.net. It also turns out that the International Space Science Organization (ISSO) run by Firmage, which includes philanthropist Henri Daiken, new energy expert, physicist Mark Cummings, and seventeen year veteran NASA scientist Creon Levitt, has requested a lecture by Dr. Mallove, to be held at 3220 Sacramento Street in San Francisco in late August. Furthermore, a viewing of the video "Cold Fusion: Fire from Water" is scheduled at that same location on Thursday, August 19.

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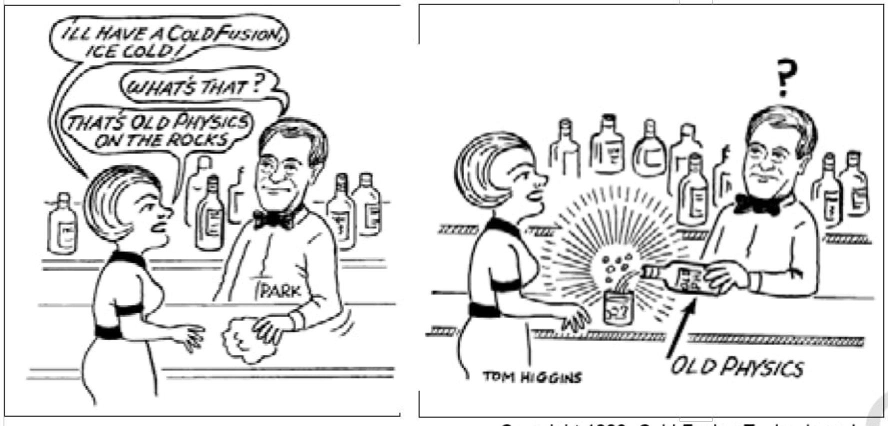
I'LL HAVE A COLD FUSION, ICE COLD!
WHAT'S THAT?
THAT'S OLD PHYSICS ON THE ROCKS.
PARK
?
TOM HIGGINS
OLD PHYSICS

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They think they can make fuel from horse manure. . . Now I don't know if your car will be able to get thrity miles to the gallon, but it's sure gonna put a stop to siphoning.

—*Billie Holliday*

Sent in by Marcello Truzzi

"The Dead Scientists Poetry Society"

IVAN PETROVICH PAVLOV GOES TO THE DOGS

BY
SUSAN SEDDON

Dog food delivery time again
Oh, how I hate this chore
Unloading vans and stacking cans
I can't take any more.

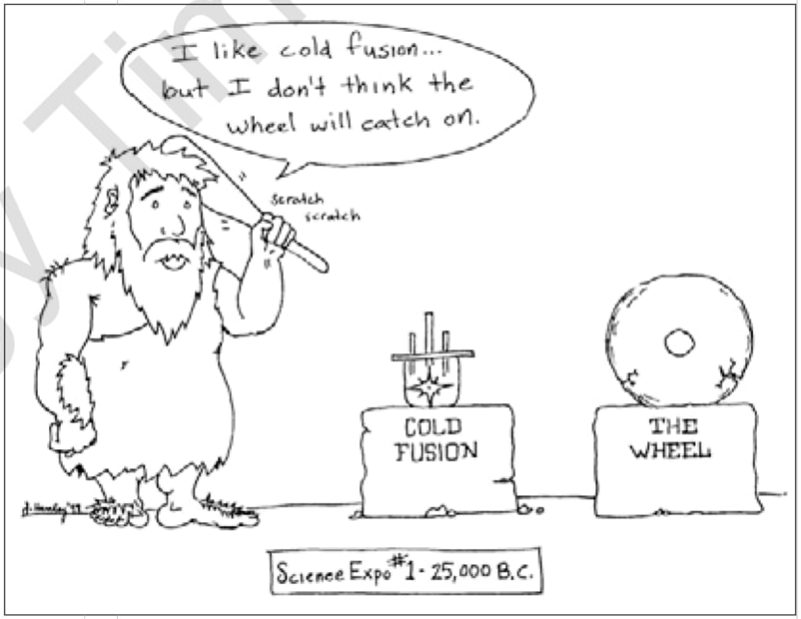
I hear the truck chug up the drive
Then stop outside my door
There goes the bell, oh, go to hell!
This task is such a bore.

Then I have to feed the mutts
I'm just a born survivor
Evading Rex's bad reflexes
And all that yuk saliva!

I've got Rex reconditioned now
With my cunning new technique
I can get him mad in just a tad
Then he's hungry, mean and sleek.

They wouldn't let me leave, you see
Said Russia needs more men in
So my savage mate now salivates
At a photo of Vladimir Lenin!

Next issue: Blaise Pascal will be guest poet.
. .in all probability!


I like cold fusion... but I don't think the wheel will catch on.
scratch scratch
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NEW ENERGY UPCOMING EVENTS

Asti International Workshop on Cold Fusion

October 22 - October 24, 1999

Hotel Villa Riccardi, Via al Monte 7, Rocca d'Arazzo, Italy. Workshop fee includes meals.

To present a paper, book a room or simply attend the workshop, please contact Bill Collis as soon as possible because space is limited.

Website: netcity.it/coldfusion
E-mail: mr.collis@physics.org

The "Millennium Congress on Energy and the Environment"

January 24-28, 2000

International Conference Centre
Geneva, Switzerland
Conference Secretariat, Rue de Varembe 3, POB 200, CH-1211, Geneva 20. Phone: +41/22 910 30 06 Fax: 22 910 30 14; e-mail: conference @cleanenergy2000.com

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CALENDAR

Asti International Workshop on Cold Fusion
October 22-24, 1999
Rocca d'Arazzo, Italy

10th Annual International Forum on New Science
October 21-24, 1999
Fort Collins, Colorado

The "Millennium Congress on Energy and the Environment"
January 24-28, 2000
Geneva, Switzerland

Climates of Change Congress
March 19-22, 2000
Victoria, B.C. Canada

ICCF-8, Eighth International Conference on Cold Fusion
May 21 - 26, 2000
Lerici (La Spezia, Italy)

8th International Conference on Cold Fusion
May 21-26, 2000

The 8th International Conference on Cold Fusion, ICCF-8, will take place in Lerici, Italy, May 21-26, 2000. The study of Cold Fusion phenomena, sometimes referred to as "New Hydrogen Energy," is progressing in numerous laboratories all around the world. More than ten years old, the field is enjoying ever more reproducible experiments and a better interaction with the scientific community. A large variety of experimental approaches will be addressed in the Conference together with theoretical attempts to interpret the observed phenomena.

List of Topics:

- Heat and related products
- Nuclear processes and transmutations
- Materials
- Innovative approaches
- Theories

The Conference will consist of oral and poster presentations. The official language of the Conference is English. Pre-registration, registration and submission of abstracts should preferably be performed using the Conference Webpage:

www.frascati.enea.it/iccf8

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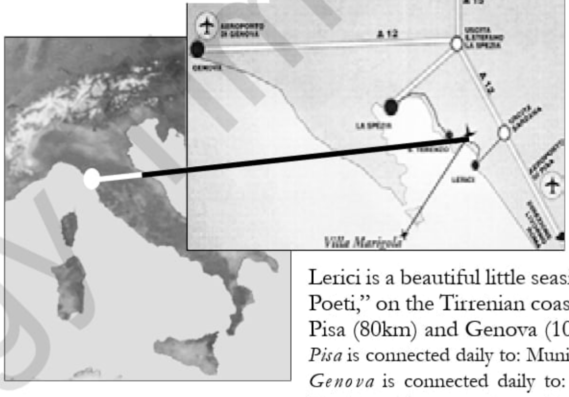


Lerici is a beautiful little seaside town in the "Golfo dei Poeti," on the Tirrenian coast. The closest airports are Pisa (80km) and Genova (100km).

Pisa is connected daily to: Munich, London and Paris

Genova is connected daily to: Frankfurt, Zurich, London, Munich. There are domestic flights connecting Pisa and Genova with Milano Malpensa and Roma Fiumicino. Once in Pisa or Genova, you can reach La Specia by train.

The trip takes about one hour. From La Spezia a bus will take you to Lerici in about fifteen minutes. A shuttle service to and from airports at the beginning and at the end of the Conference is being considered. More detailed information about travel and site are available on the Conference webpage.

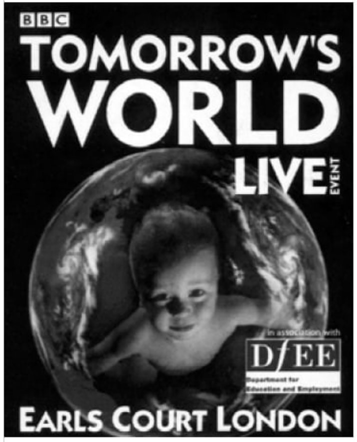
Organized by: The Italian Agency for New Technologies, Energy and the Environment
With the sponsorship of :
Consiglio Nazionale delle Ricerche (CNR), Istituto Nazionale di Fisica Nucleare (INFN), and the Società Italiana di Fisica (SIF)

VILLA MARIGOLA is situated on a hill just outside the village of Lerici. Shuttle service will be available for the five days of the meeting to/from Lericito from Villa Marigola. The mild Liguirian climatic conditions are extremely suitable to activities all year long. The hotel owners guarantee that this is a place where work can be mixed with moments of peaceful relaxation and where excellent food can be enjoyed.

VILLA MARIGOLA, Lerici (La Specia, Italy)

Tomorrow's World Live

Soo Seddon



I paid a visit to Earls Court to check out the third annual "Tomorrow's World Live" event hosted by the BBC in association with the Department for Education and Employment. The enormous London exhibition center was crammed almost to the rafters with impressive state-of-the-art displays representing the latest innovations in the fields of communications, computing, engineering, transport, health, and the environment.

A Eurofighter Typhoon "guarded" the entrance to the main hall. This highly-advanced combat aircraft, being jointly developed by the aviation industries of the U.K., Germany, Italy, and Spain, can reach speeds of over 1500 mph—the equivalent of flying from London to Paris in eight minutes. Neighboring aeronautics stands couldn't boast mega-buck international collaboration like Project Eurofighter, but were equally impressive in their own way.

Ben Jarvis and Richard Osborne, both from the south of England, were seeking sponsors to help with their planned escape from Earth. For over a decade their organization, MARS (Middlesex Advanced Rocketry Society), has been at the forefront of the amateur space race by flying record-breaking rockets "faster, higher and cheaper" than any other rocketry society in the U.K. Their latest project, Deimos 3, is a bid to put a British amateur rocket into space early in the new millennium. Their ultimate aim is to refine the technology sufficiently to enable them to launch satellites into orbit at a fraction of the present cost. The Society's message is that you don't have to be a rocket scientist to build a rocket, and they cordially invite donations and/or practical assistance from members of the general public to help launch their "Space program of the people, by the people, for the people." Further information is available on their website at http://www.mars.org.uk/.

One of the most intriguing sections at the exhibition housed over 250 inventors displaying their newly-patented wares. Although, for the main part, a diverse range of inventions, I couldn't help but notice that variations on a similar theme appeared to dominate in the "domestic" sector.

The event could almost have been re-titled the "Build a Better Bathroom" exhibition, such was the range of innovative ideas for the improvement of that most basic of household necessities, the humble lavatory. A Cambridge inventor had produced the "Odorless Superloo" which extracted unpleasant fragrances at source by means of a vacuum pump built into the bowl, thereby converting the flush waterways into airways.

There were many variations on the theme of more eco-friendly, water-saving mechanisms and one inventor had combined lavatory and bidet in one which, he assured me, used a completely different water supply for the secondary process. Apparently, I was the first visitor to his stand to ask him about

Live continued on page 46

Millennium Media Watch

Energy News

Some recent developments on the fuel cell front. . .
Shell Hydrogen, in collaboration with a Michigan-based materials technology company, Energy Conversion Devices, is investigating methods for the storage of solid hydrogen. If successful, this technology would eliminate the need for on-board reformers in fuel cell vehicles.

A company named ZeTek Power recently announced its development of a cobalt catalyst for fuel cells which, they say, will greatly reduce production costs by replacing the expensive platinum currently in use.

Europe's second largest island, Iceland, is working towards abandoning fossil fuels forever. They are hoping to utilize their most plentiful resource, water, to cater to all of their energy needs within the next fifteen to twenty years.

Most of their electricity is already obtained from hydroelectric plants, and they make use of natural geothermal energy for heating. The Icelandic Hydrogen Committee is now working in association with Daimler-Chrysler, Shell, and Ballard to make their road vehicles and fishing vessels all electric and powered by fuel cells. They also plan, eventually, to export their surplus hydrogen to the rest of the world.

In Japan, Nasda scientists plan to put a giant hankie into space. They are experimenting with furoshiki, a light silky fabric generally used for handkerchiefs, which they intend to make into a solar sail which could provide electrical power to spacecraft exploring the outer reaches of the solar system. The proposed 1,500 square foot sail, when lined with specially-designed solar cells, should generate 2.5 megawatts of power. They hope to have a demonstration model in orbit within the next two years.

No Energy News?

Millennium baby competition organizers are baffled. Despite offering enormous cash sums to the first mum to give birth on January 1, 2000, they report a much lower level of contest entrants than expected.

On April 9, the date considered the most appropriate on which to conceive a millennium baby, broody couples in New Zealand were treated to free accommodations at a luxury hotel in exchange for their participation in a live Internet show.

In the U.K., however, things were done with a little more discretion, but there was a TV special on the 9th after which the network aired non-stop romantic movies. British event sponsors of "Birthrace 2000" recently stated that they now have ten potential millennium babies on the way and they intend to have cameras set up in maternity wards throughout the country on New Year's Eve.

But their camera-persons might have to don masks and gowns and get hurriedly scrubbed-up if the threatened walk-out by English midwives goes ahead. Maternity hospital staff are angry that their employers refuse to pay them a higher rate for New Year's Eve work when just about everyone else in the U.K. appears to be receiving offers of five times their normal pay rate for one night's work!

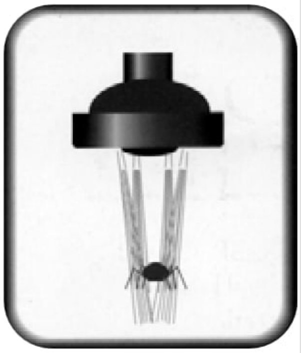
And, of course, there's still that millennium bug to worry about. . .

❐❐❐

Live continued from page 45

that! My favorite was a kind of Heath Robinson device for resolving the age old male/female conflict of vertical versus horizontal loo seats. The inventor had rigged up a foot pedal to take the strain out of the raising and lowering process.

But what to do about the other major "smallest room" trauma . . . the huge, hairy spiders that appear to like bathtubs so much? An Irish company had the solution to that with their "Spider Catcher," a long pole with a series of fine bristles at one end. The helpless arachnid is very gently held in place by the gripping action of the bristles which are activated by a trigger on the handle. It can then be released unharmed back into the wild (or over the neighbor's fence). One U.K. women's magazine had given the device a gleeful "thumbs-up," stating: "This contraption could make men completely redundant."



The "pets" section housed some interesting new gadgets for rendering canine foul waste less foul and there was a cat flap which automatically sprayed the incoming feline with anti-flea treatment.

On the more hi-tech front, a Bristol company was selling tiny electronic devices which, when clipped onto the aerial of a mobile phone, they claimed, reduced radiation entering the head by 95%.

Even more life-enhancing (well to us Brits, at least) was the world's first non-drip teapot. Invented by twenty-three-year-old Damini Kumar, a protégée of clockwork radio inventor Trevor Baylis. As all compulsive tea-drinkers will appreciate, the leaky spout problem has cast an irritating cloud over our afternoon beverage break for generations. Damini's invention formed a significant part of her Masters degree in Engineering Product Design at London's South Bank University and can, she says, be applied to any pouring technology.

Mr. Berwick from Fife, Scotland, had invented a device to ease the stresses and strains of adding keys onto a keyring: "no more broken fingernails . . ." his advertisement proudly proclaimed ". . . keep this little gizmo on your keyring and never have difficulties again." When I asked him how I was supposed to get the gizmo itself onto my keyring without breaking fingernails, he thought for a while then, in true entrepreneurial fashion declared, "That's easy, you buy two of them!" One thing's for certain—you can't keep a good inventor down!

◆◆◆

He that can't endure the bad will not live to see the good.

—Yiddish Proverb, Submitted by Moses Bernard

Standards

Author Unknown

The U.S. Standard railroad gauge (distance between the rails) is 4 feet, 8.5 inches. That's an exceedingly odd number.

Why was that gauge used?

Because that's the way they built them in England, and the U.S. railroads were built by English expatriates.

Why did the English people build them like that?

Because the first rail lines were built by the same people who built The pre-railroad tramways, and that's the gauge they used.

Why did "they" use that gauge then?

Because the people who built the tramways used the same jigs and tools that they used for building wagons, which used that wheel spacing.

Why did the wagons use that odd wheel spacing?

Well, if they tried to use any other spacing the wagons would break on some of the old, long distance roads, because that's the spacing of the old wheel ruts.

So who built these old rutted roads?

The first long distance roads in Europe were built by Imperial Rome for the benefit of their legions. The roads have been used ever since.

And the ruts?

The initial ruts, which everyone else had to match for fear of destroying their wagons, were first made by Roman war chariots. Since the chariots were made for or by Imperial Rome they were all alike in the matter of wheel spacing.

Thus, we have the answer to the original question. The United States standard railroad gauge of 4 feet, 8.5 inches derives from the original specification for an Imperial Roman army war chariot. Specs and Bureaucracies live forever.

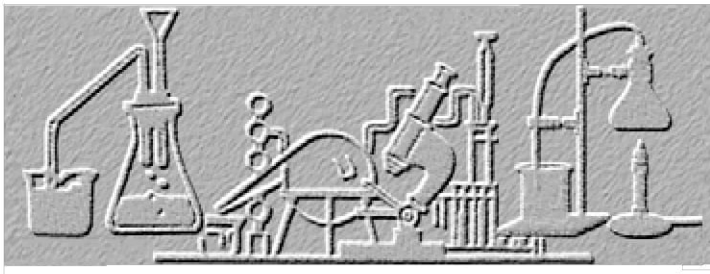
So, the next time you are handed a specification and wonder if the person who came up with it really knew what he or she was doing, you may be exactly right, because the Imperial Roman chariots were made to be just wide enough to accommodate the back ends of two war horses.

Now the twist to the story. . .

There's an interesting extension of the story about railroad gauge and horses' behinds. When we see a Space Shuttle sitting on the launch pad, there are two big booster rockets attached to the sides of the main fuel tank. These are the solid rocket boosters, or SRBs. The SRBs are made by Thiokol at a factory in Utah. The engineers who designed the SRBs might have preferred to make them a bit fatter, but the SRBs had to be shipped by train from the factory to the launch site. The railroad line to the factory runs through a tunnel in the mountains. The SRBs had to fit through that tunnel. The tunnel is slightly wider than a railroad track, and the railroad track is about as wide as two horses' behinds. So a major design feature of what is arguably the world's most advanced transportation system was determined by the width of a "Horse's Behind"!

USA
NASA
Discovery

❑❑❑



EXPERIMENTER'S CORNER

Notes on Experiments on Radioactivity in Welds

George Sassoon*

Reading the literature on cold fusion, I came across reports that electric arcs could cause transmutations and other nuclear reactions. Being in possession of an electric arc welder, quantities of scrap steel, and Geiger-Mueller counting equipment, I decided to do some experiments.

The welder is an Italian-made Cebora, type 1722, maximum welding current 140 amps. The radiation detectors are from Aware Electronics, Wilmington, Deleware, type RM-80. These use a pancake-type tube with a mica alpha window on one side, about 30 mm diameter. At this location in the west of Scotland near sea level, the cosmic background count rate is about 32 counts/min, corresponding to 9 microRoentgens/hour (μR/hr) according to the tube's certified calibration. This is with the tube on its side, as it is normally operated. The background is slightly higher with the tube flat, as the cosmic radiation is principally from a vertical direction. See website www.aw-el.com for details of the detectors.

The steel pieces used are old farm gate hinges, strips about 50 mm wide by 8 mm thick. There is no detectable radiation from them before welding. The welding rods used are from various sources—I can supply details if required. The flux coating of these contains minerals which are slightly radioactive. I have had them for some time, so there can be no short-lived radionuclides in them.

Experimental Method

The RM-80 is clamped to a table on its side, with an unwelded portion of a gate hinge close to its alpha window. The computer logging program provided by Aware Electronics is started and run for a few hours to establish background. The raw data file is a sequence of numbers, which are counts for successive minutes with date/time and other data.

The hinge is then removed, and any rust cleaned off with an angle grinder. A large flat weld is then made, about the diameter of the tube's window, the slag knocked off, and the weld quenched in water. It is then clamped in position close to the alpha window and the program left to run for as long as possible.

The computer program allows a curve to be displayed giving successive averages over periods of an integral number of minutes. Initially, this is set to six minutes (0.1 hr) but after some time it is increased to one hour or more.

The raw data files can be converted to ASCII for importation into spreadsheets such as Excel, and also graphs in GIF format with varying periods of radiation level averaging.

Results

In every case, when the weld is placed by the tube's alpha window, the count rate rises from the background 9 to typically 23 μR/hr, and then starts to fall. It reaches a minimum after a few hours, and then starts to rise again, tending exponentially towards a limit of about 25 μR/hr with a half-life of about 150 hours. This latter part of the curve is like the charging of a capacitor via a resistor from a fixed voltage source.

Putting various pieces of paper, aluminum sheet, etc. between the weld and the detector, it appears that the radiation is partly alpha but primarily beta or low-energy gamma. There

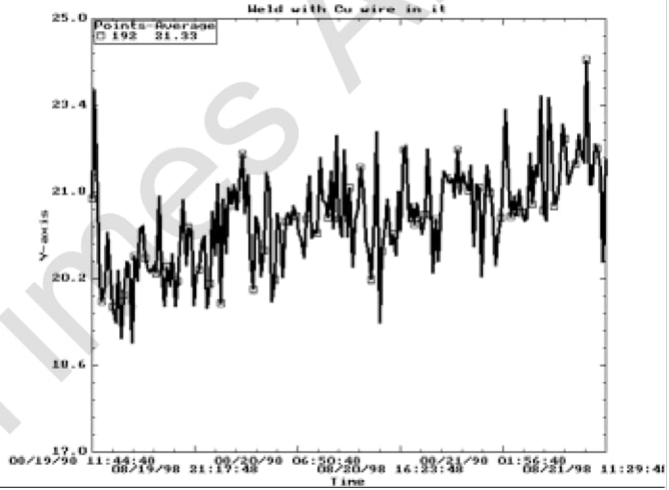


Figure 1. The forty-eight-hour record averages radiation over fifteen-minute periods and shows the initial sharp rise and fall, then the start of the slow rise.

Figures 1 and 2 show the first 48 and 600 hours of radiation level from a typical weld. In this case I added some copper wire to the weld, which made no significant difference. The Y-axis gives radiation level in mR/hr, the local cosmic background being about 9 mR/hr.

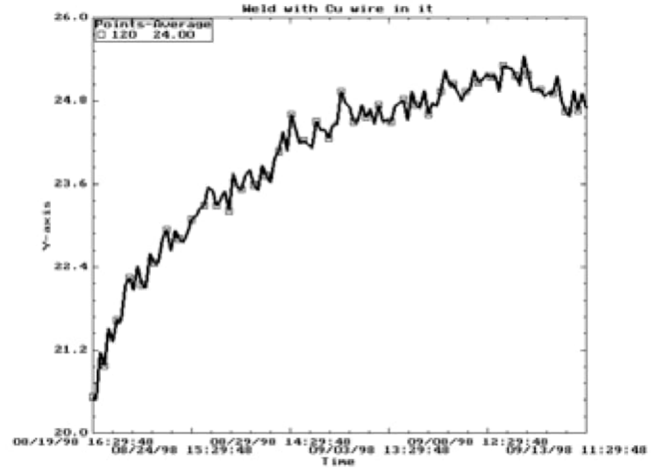


Figure 2. The 600-hour record shows the development of the slow rise, but the initial fast rise and fall is not visible as the averaging period in this record is five hours. The Y-axis gives radiation level in μR/hr, the local cosmic background being about 9 μR/hr.

is no detectable radiation through the thickness of the hinge (about 8 mm) behind the weld.

The thickness of the weld does not appear to matter, implying that the radiation is from the surface and not the bulk of the weld. I have tried thick welds using 10 rods (20-30 amp types) and thin ones using only two, and the results are not significantly different.

Due to the extremely low radiation levels involved, the curves obtained are noisy. One thing I have discovered is that the windows of the room should be left open, to dispel any radon or thoron. Even very low levels of these gases can affect the results. The house is in a low-radon area, but some of the modern materials used (such as plasterboard) exude significant amounts of the gases.

I hope to be able to obtain some welding rods with non-radioactive flux (or none at all) for further experiments. With the present rods, the flux is necessary. If it is scraped off, the rods stick to the workpiece, and a lasting arc cannot be struck.

Analysis

Undoubtedly some of the radioactive flux coating of the welding rods will remain in the weld, causing the initial rise in observed radiation. But why should it change thereafter? In particular, why should it drop off after the initial rise and then, after some hours, start to rise again? Is there some diffusion process going on? And why should broadly similar results be obtained in all the welds (about a dozen) that I have done?

Possible Explanation

Supposing that the electric arc generates neutrons, one might find some neutron-activation of the steel. Suppose that this transmutes the iron into a radioactive isotope with a half-life of about twelve hours, and another with a half-life of 150 hours, which is an alpha-emitter and decays in turn into some long-lived radioactive isotope.

We would see the initial decay of the twelve-hour isotope, but not of the 150-hour one, as the alphas would not be able to escape the steel. But we would see the decay product of the 150-hour isotope, a beta/gamma emitter, and this would cause the slow rise over 500 hours which is observed. I have done a fair simulation of this curve using Excel.

The flux coating of the rods is a mixture of various minerals and a binder, and the elements in these substances could also be neutron-activated. The slight radioactivity of the flux could be due to potassium, as naturally-occurring K is slightly radioactive (K-40, 0.0012%, half-life 1.28×10^{9} years, according to my *CRC Handbook of Chemistry and Physics*).

I would be most grateful if someone could repeat these simple experiments and, if possible, use a spectrum analyzer on the radiation. I would also like to know what does in fact happen when Fe, K, etc. are bombarded with neutrons, and the half-lives of any radionuclides generated.

[**Editor's Note:** A brief telephone report by another subscriber *may* suggest a mundane explanation of this anomalous radioactivity—pre-existing radioactivity in the grinding wheel.]*—EFM*

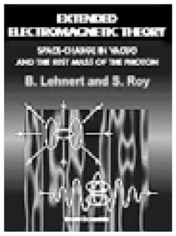
About the Author

George Sassoon was born in London in 1936 and educated at the Oundle School and Cambridge University, where he obtained a science degree. He worked as an electronics designer in industry and is now semi-retired and living on a farm in the Isle of Mull, Scotland.

*Lochbuie, Isle of Mull, Scotland, PA62 6AA; e-mail: geosas@aol.com

BOOK REVIEW

Hal Fox*


EXTENDED ELECTROMAGNETIC THEORY
SPACE-CHARGE IN VACUO AND THE REST MASS OF THE PHOTON
B. Lehnert and S. Roy

Bo Lehnert and Sisir Roy, *Extended Electromagnetic Theory: Space-Charge in Vacuo and the Rest Mass of the Photon,* World Scientific Series in Contemporary Chemical Physics—Vol 16, Published by World Scientific, River Edge, New Jersey 07661, c1998, 160 pages, illustrations, 100 references, indexed, ISBN 981-02-3395-7.

In this timely monograph, the authors develop important equations using modified forms of Maxwell's equations. Some of the conditions developed are for a non-zero divergence of the electric field and for non-zero mass of the photon coupled with the concept of a non-zero electrical conductivity of the vacuum. The end results are some important new concepts that can be tested in the laboratory. The conditions by which the photon had been previously believed to have zero mass are carefully presented and shown not to have a binding constraint. By demonstrating that the photon has a non-zero mass, it is proven that such photons can interact with a non-zero conductivity of space with the result that photons can lose energy. The energy lost is shown to be reasonably close to the measured low-frequency microwave radiation which appears to penetrate all space. The end result is consistent with observed experimental evidence: can be shrunk to approach the concept of a point charge; fits much of the data of the neutrino; appears consistent with an earlier proposed string model of hadron structures; and leads to further development.

In addition, new types of electromagnetic wave phenomena can be described. This leads to a better method to handle both wave and particle behavior and can explain total reflection of light. Both longitudinal and transverse waves are handled. The new approach developed may lead to a deeper insight into the transition of a beam of photons to macroscopic light waves.

Of most interest to this reviewer is the development of the concept of "tired" light—light which loses some of its energy without distortion so that the visual field (such as photos of distant stars) is essentially unaffected. It is shown that the photons can have mass and that the interaction of such a non-zero mass of photons with space-charge in the vacuum of space can result in lower photon energy (a red shift). Further, it is shown that the lost energy appears to closely match the observed microwave energy in space. Thus the two bastions of evidence for the Big Bang (red shift and microwave radiation) are analytically explained. This analytical development is sufficient reason for any thoughtful scientist to ensure that this book is purchased and read. The list price ($32.00) should not be a barrier for such an important contribution to the advancement of our understanding of new aspects of electromagnetic radiation.

[Note: The findings of this book (concerning the red shift and the background microwave radiation) support a similar result. See: Anastasovski, P., Fox, H., and Shoulders, K. 1996. "A New Approach to the Cosmic Red-Shift and to the Cosmic Microwave Sources," *Journal of New Energy*, **1**, 2, p. 79-87.]

About the Author

Hal Fox is an unretired 75-year-old scientist. He and his friends started the Fusion Information Center at the University of Utah Research Park. He and his staff have collected numerous papers on cold fusion from over 200 laboratories throughout the world. From those grew two publications—*New Energy News* and *Journal of New Energy.*

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The Warlock's Wheel

Jeffery D. Kooistra

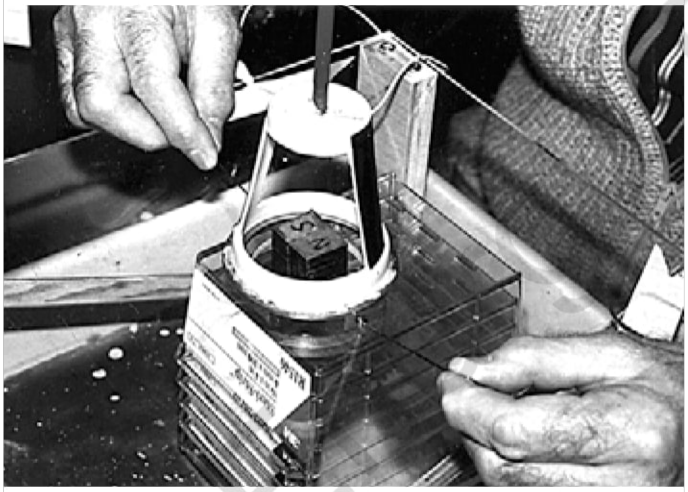
The Warlock's Wheel is a variant of the Marinov Motor, which I discovered in the spring of 1998. Issue No. 19 of *Infinite Energy* contains an article by me entitled "How to Think About the Marinov Motor." In the letters section of Issue No. 20 I provided an update which should have been called "Oops—Never Mind."

Since that letter is the last thing that's been published by me about my Marinov Motor work, and since a great deal of work continues, I decided to present the material mentioned in that letter in a more formal way before going on to later work.

Simply put, the Warlock's Wheel is a version of the Marinov Motor in which both the magnet pair (or torus) and the ring are free to rotate independently of each other. Why would one wish to build such a thing?

It had been assumed by Dr. Thomas Phipps, Jr., Tom Ligon, and me right from the start that if the ring would rotate as Marinov said it would, then if one fixed the ring and allowed the magnets to turn instead, which was much easier to do experimentally, this would tell us something about the forces being exerted on the ring by the magnets. We were mistaken.

I'm not going to go into any details about what I now think is going on with this motor. I leave that for a later, extensive report. This is just a description of observations made on a single version of the Marinov Motor that I put together. To date, I'm still the only person I know of who has built one like this.



In the above picture you see the copper ring and the magnet pair tucked away inside it—N and S poles clearly marked. The ring is attached to an ordinary styrofoam coffee cup and projecting upward from the cup is a hollow straw. The straw has two thin strands of monofilament line attached to it on the top (not shown), and these connect to a hook on the ceiling. This forms a low-torque suspension for the ring. Also, you see the hands of Dr. Phipps holding two short rods of tungsten which he uses to touch two beads of mercury. The mercury beads act as brushes to bring current in to the ring. The rod in Phipps' left hand (foreground) is connected to the + terminal of a pair of 9-volt batteries wired in parallel.

Now, this is not a photo of an actual Warlock's Wheel since the magnet pair is not free to rotate. The Warlock's Wheel requires a couple of changes, discussed below. (These changes make Warlock's Wheel photos a bit too cluttered to sort out details in monochrome photos.)

Look at the picture again. Notice that the mercury beads are outside the ring. This is critical. Once the circuit is closed, the ring will begin to rotate clockwise as seen from above.

Notice that the magnet pair—these are just two stacks of twenty wafer-sized rare earth magnets—have no keepers. In other words, this is a very flux-leaky arrangement. Much of the theoretical Marinov Motor work centered on the prediction of forces that would appear even if the torus (or magnet pair) contained its flux perfectly. This version of the Marinov Motor does not do that and, thus, cannot answer questions dealing with flux-containing toroids. This version of the motor, I now believe, is actually an interesting variant of a homopolar motor, previous comments and papers by this author notwithstanding. (I am not going to discuss in this article *why* I now think this version is a homopolar motor. That's for another time.)

Going back to the picture, suppose that the ring is now fixed to the table, and that the magnet pair is suspended by a thin monofilament line going up through the straw in the cup—which is what is done for Warlock's Wheel work. In that case, closing the circuit results in the magnet pair starting to rotate counterclockwise. If the circuit is closed for just a fraction of a second—in other words, a single pulse is delivered—then the magnets rotate, due to their own momentum, for quite some time with the current off. Here's where it starts to get weird.

Suppose that the magnets are again fixed in place, but this time the mercury beads are positioned on the *inside* of the copper ring. With steady hands, Dr. Phipps slides the tungsten rods under the ring to touch the beads and close the circuit with the battery. This time the ring rotates counterclockwise as seen from above. However, if we fix the ring and attach the leads of the battery to the inside of the ring, and allow the magnet pair to turn, the magnets *still* would go counterclockwise. Now we're ready for the Warlock's Wheel.

In the Warlock's Wheel, both the ring and the magnets are free to turn independently of each other. The ring is suspended as described above, and the magnet pair is suspended with a length of monofilament threaded through the straw. One other important experimental modification must be made—the rotation of the magnets is damped. This is because it takes time for the ring to build up speed, while the magnet pair responds to the current very quickly.

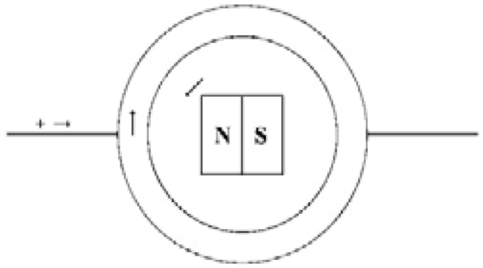


Figure 1. Brushes to outside of ring. Ring rotates clockwise. Magnet pair rotates counterclockwise.

Look at Figure 1 and note the typical starting position. If the magnets are rotated a quarter turn counterclockwise, into the null position, then nothing happens when current is applied. The purpose of the damping is simply to slow down the magnet pair so there is time to see the interesting effect. For damping, I used very cold Log Cabin pancake syrup in a well into which the

Warlock's Wheel continues on page 66

THE SPEED OF GRAVITY: WHAT THE EXPERIMENTS SAY

Tom Van Flandern*

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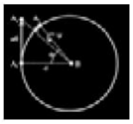
Abstract



Standard experimental techniques exist to determine the propagation speed of forces. When we apply these techniques to gravity, they all yield propagation speeds too great to measure, substantially faster than light speed. This is because gravity, in contrast to light, has no detectable aberration or propagation delay for its action, even for cases (such as binary pulsars) where sources of gravity accelerate significantly during the light time from source to target. By contrast, the finite propagation speed of light causes radiation pressure forces to have a non-radial component causing orbits to decay (the "Poynting-Robertson effect"); but gravity has no counterpart force proportional to v/c to first order. General relativity (GR) explains these features by suggesting that gravitation (unlike electromagnetic forces) is a pure geometric effect of curved space-time, not a force of nature that propagates. Gravitational radiation, which surely does propagate at light speed but is a fifth order effect in v/c, is too small to play a role in explaining this difference in behavior between gravity and ordinary forces of nature. Problems with the causality principle also exist for GR in this connection, such as explaining how the external fields between binary black holes manage to continually update without benefit of communication with the masses hidden behind event horizons. These causality problems would be solved without any change to the mathematical formalism of GR, but only to its interpretation, if gravity is once again taken to be a propagating force of nature in flat space-time with the propagation speed indicated by observational evidence and experiments: not less than 2 x 10^{10}c. Such a change of perspective requires no change in the assumed character of gravitational radiation or its light speed propagation. Although faster-than-light force propagation speeds do violate Einstein special relativity (SR), they are in accord with Lorentzian relativity, which has never been experimentally distinguished from SR—at least, not in favor of SR. Indeed, far from upsetting much of current physics, the main changes induced by this new perspective are beneficial to areas where physics has been struggling, such as explaining experimental evidence for non-locality in quantum physics, the dark matter issue in cosmology, and the possible unification of forces. Recognition of a faster-than-light speed propagation of gravity, as indicated by all existing experimental evidence, may be the key to taking conventional physics to the next plateau.

Introduction

The most amazing thing I was taught as a graduate student of celestial mechanics at Yale in the 1960s was that all gravitational interactions between bodies in all dynamical systems had to be taken as instantaneous. This seemed unacceptable on two counts. In the first place, it seemed to be a form of "action at a distance." Perhaps no one has so elegantly expressed the objection to such a concept better than Sir Isaac Newton: "That one body may act upon another at a distance through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to the other, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it."[6] But mediation requires propagation, and finite bodies should be incapable of propagating at infinite speeds since that would require infinite energy. So, instantaneous gravity seemed to have an element of magic to it.

The second objection was that we had all been taught that Einstein's special relativity (SR), an experimentally well-established theory, proved that nothing could propagate in forward time at a speed greater than that of light in a vacuum. Indeed, as astronomers we were taught to calculate orbits using instantaneous forces; then extract the position of some body along its orbit at a time of interest, and calculate where that position would appear as seen from Earth by allowing for the finite propagation speed of light from there to here. It seemed incongruous to allow for the finite speed of light from the body to the Earth, but to take the effect of Earth's gravity on that same body as propagating from here to there instantaneously. Yet that was the required procedure to get the correct answers.

These objections were certainly not new when I raised them. They have been raised and answered thousands of times in dozens of different ways over the years since general relativity (GR) was set forth in 1916. Even today in discussions of gravity in USENET newsgroups on the Internet, the most frequently asked question and debated topic is, "What is the speed of gravity?" It is only heard less often in the classroom because many teachers and most textbooks head off the question by hastily assuring students that gravitational waves propagate at the speed of light, leaving the firm impression, whether intended or not, that the question of gravity's propagation speed has already been answered.

Yet, anyone with a computer and orbit computation or numerical integration software can verify the consequences of introducing a delay into gravitational interactions. The effect on computed orbits is usually disastrous because conservation of angular momentum is destroyed. Expressed less technically by Sir Arthur Eddington, this means: "If the Sun attracts Jupiter towards its present position S, and Jupiter attracts the Sun towards its present position J, the two forces are in the same line and balance. But if the Sun attracts Jupiter toward its previous position S', and Jupiter attracts the Sun towards its previous position J', when the force of attraction started out to cross the gulf, then the two forces give a couple. This couple will tend to increase the angular momentum of the system, and, acting cumulatively, will soon cause an appreciable change of period, disagreeing with observations if the speed is at all comparable with that of light."[3] (See Figure 1.)

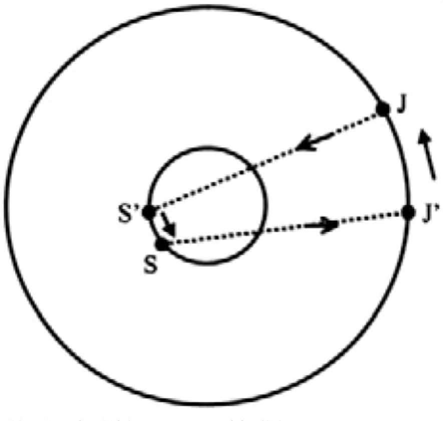


Figure 1. Orbits are unstable if forces propagate with finite speed.

Indeed, it is widely accepted,

even if less widely known, that the speed of gravity in Newton's Universal Law is unconditionally infinite.[11] This is usually not mentioned in proximity to the statement that GR reduces to Newtonian gravity in the low-velocity, weak-field limit because of the obvious question it begs about how that can be true if the propagation speed in one model is the speed of light, and in the other model it is infinite.

The same dilemma comes up in many guises: Why do photons from the Sun travel in directions that are not parallel to the direction of Earth's gravitational acceleration toward the Sun? Why do total eclipses of the Sun by the Moon reach maximum eclipse about forty seconds before the Sun and Moon's gravitational forces align? How do binary pulsars anticipate each other's future position, velocity, and acceleration faster than the light time between them would allow? How can black holes have gravity when nothing can get out because escape speed is greater than the speed of light?

Herein we will examine the experimental evidence bearing on the issue of the speed of propagation of gravity. By gravity, we mean the gravitational "force" from some source body. By force, we mean that which gives rise to the acceleration of target bodies through space. (Note: Orbiting bodies do accelerate through space even if gravity is geometry and not a true force. For example, one spacecraft following another in the same orbit can stretch a tether between the two. The taut tether then describes a straight line, and the path of both spacecraft will be curved with respect to it.) We will examine the explanations offered by GR for these phenomena. And we will confront the dilemma that remains when we are through: whether to give up our existing interpretation of GF, or the principle of causality.

Propagation Delay Versus Aberration

To understand how propagation speeds of phenomena are normally measured, it will be useful to discuss propagation or transit delay and aberration, and the distinction between them. The points in this section are illustrated in Figure 2.

In the top half of the figure, we consider the view from the source. A fixed source body on the left (for example, the Sun) sends a projectile (the arrow, which could also be a photon) toward a moving target (for example, the Earth). Infinitely far to the right are shown a bright (large, five-pointed) star and a faint (small, four-pointed) star, present to define directions in space. Because of transit delay, in order to hit the target, the source body must send the projectile when it is seen in the direction of the faint star, but send it toward the direction of the bright star, leading the target. The tangent of the lead angle (the angle between the two stars) is the ratio of the tangential target speed to the radial projectile speed. For small angles, this ratio equals the lead angle in radians.

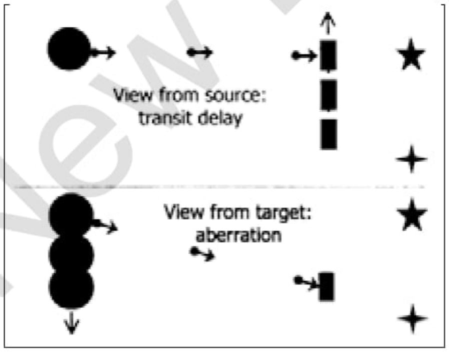


Figure 2. Top shows transit delay: source fixed, target moves. Bottom shows aberration: source moves, target fixed.

In the bottom half of the figure, we consider the view from the target, which will consider itself at rest and the source moving. By the principle of relativity, this view is just as valid since no experiment can determine which of two bodies in uniform, linear relative motion is "really moving" and which is not. The projectile will be seen to approach from the retarded position of the source, which is the spatial direction headed toward the faint star. The angle between the true and retarded positions of the source, which equals the angle between the two stars, is called "aberration." It will readily be recognized as the same angle defined in the first view due to transit delay.

Indeed, that is generally true: The initial and final positions of the target as viewed from the source differ by the motion of the target during the transit delay of the projectile. The same difference between initial and final positions of the source as viewed from the target is called the angle of aberration. Expressed in angular form, both are equal, and are manifestations of the finite propagation speed of the projectile as viewed from different frames. So the most basic way to measure the speed of propagation of any entity, whether particle or wave or dual entity or neither, is to measure transit delay or, equivalently, the angle of aberration.

Fact: Gravity Has No Aberration

The effect of aberration on orbits is not seen.

As viewed from the Earth's frame, light from the Sun has aberration. Light requires about 8.3 minutes to arrive from the Sun, during which time the Sun seems to move through an angle of 20 arc seconds. The arriving sunlight shows us where the Sun was 8.3 minutes ago. The true, instantaneous position of the Sun is about 20 arc seconds east of its visible position, and we will see the Sun in its true present position about 8.3 minutes into the future. In the same way, star positions are displaced from their average position by up to 20 arc seconds, depending on the relative direction of the Earth's motion around the Sun. This well-known phenomenon is classical aberration and was discovered by the astronomer Bradley in 1728.

Orbit computations must use true, instantaneous positions of all masses when computing accelerations due to gravity for the reason given by Eddington.[3] When orbits are complete, the visible position of any mass can be computed by allowing for the delay of light traveling from that mass to Earth. This difference between true and apparent positions of bodies is not merely an optical illusion, but is a physical difference due to transit delay that can alter an observer's momentum. For example, small bodies such as dust particles in circular orbit around the Sun experience a mostly radial force due to the radiation pressure of sunlight. But because of the finite speed of light, a portion of that radial force acts in a transverse direction, like a drag, slowing the orbital speed of the dust particles and causing them to eventually spiral into the Sun. This phenomenon is known as the Poynting-Robertson effect.

If gravity were a simple force that propagated outward from the Sun at the speed of light, as radiation pressure does, its mostly radial effect would also have a small transverse component because of the motion of the target. Analogous to the Poynting-Robertson effect, the magnitude of that tangential force acting on the Earth would be 0.0001 of the Sun's radial force, which is the ratio of the Earth's orbital speed (30 km/s) to the speed of this hypothetical force of gravity moving at light-speed (300,000 km/s). It would act continuously, but would tend to speed the Earth up rather than slow it down because gravity is attractive and radiation pressure is repulsive. Nonetheless, the net effect of such a force would be to double the Earth's distance from the Sun in 1200 years. There can be no doubt from astronomical observations that no such force is acting. The computation using the instantaneous positions of Sun and Earth is the correct one. The computation using retarded positions is in conflict with observations. From the absence of such an effect, Laplace set a lower limit to the speed of propagation of classical gravity of about $10^8\,c$, where c is the speed of light.[9]

In the general case, let v_g be the speed of propagation of gravitational force, and let a_0 be the initial semi-major axis at time t_0 of an orbiting body in a system where the product of the gravitational constant and the total system mass is μ. Then the following formula, derived from the ordinary perturbation formulas of celestial mechanics,[2] allows us to compute the semi-major axis a at any other time t:

$$a^2 = a_0^2 + 4\mu(t-t_0)/v_g \quad (1)$$

We will use this formula later to set limits on v_g.

Gravity and light do not act in parallel directions.

There is no cause to doubt that photons arriving now from the Sun left 8.3 minutes ago, and arrive at Earth from the direction against the sky that the Sun occupied that long ago. But the analogous situation for gravity is less obvious, and we must always be careful not to mix in the consequences of light propagation delays. Another way (besides aberration) to represent what gravity is doing is to measure the acceleration vector for the Earth's motion, and ask if it is parallel to the direction of the arriving photons. If it is, that would argue that gravity propagated to Earth with the same speed as light; and conversely.

Such measurements of Earth's acceleration through space are now easy to make using precise timing data from stable pulsars in various directions on the sky. Any movement of the Earth in any direction is immediately reflected in a decreased delay in the time of arrival of pulses toward that direction, and an increased delay toward the opposite direction. In principle, Earth's orbit could be determined from pulsar timings alone. In practice, the orbit determined from planetary radar ranging data is checked with pulsar timing data and found consistent with it to very high precision.

How then does the direction of Earth's acceleration compare with the direction of the visible Sun? By direct calculation from geometric ephemerides fitted to such observations, such as those published by the U.S. Naval Observatory or the Development Ephemerides of the Jet Propulsion Laboratory, the Earth accelerates toward a point 20 arc seconds in front of the visible Sun, where the Sun will appear to be in 8.3 minutes. In other words, the acceleration now is toward the true, instantaneous direction of the Sun now, and is not parallel to the direction of the arriving solar photons now. This is additional evidence that forces from electromagnetic radiation pressure and from gravity do not have the same propagation speed.

> ***An old saying among Air Force bomber pilots: "You get the most flak when you're directly over the target."***

The solar eclipse test.

Yet another manifestation of the difference between the propagation speeds of gravity and light can be seen in the case of solar eclipses.[15] The Moon, being relatively nearby and sharing the Earth's 30 km/s orbital motion around the Sun, has relatively little aberration (0.7 arc seconds, due to the Moon's 1 km/s orbital speed around Earth). The Sun, as mentioned earlier, has an aberration of just over 20 arc seconds. It takes the Moon about 38 seconds of time to move 20 arc seconds on the sky relative to the Sun. Since the observed times of eclipses of the Sun by the Moon agree with predicted times to within a couple of seconds, we can use the orbits of the Sun and the Moon, near times of maximum solar eclipse, to compare the time of predicted gravitational maximum with the time of visible maximum eclipse.

In practice, the maximum gravitational perturbation by the Sun on the orbit of the Moon near eclipses may be taken as the time when the lunar and solar longitudes are equal. Details of the procedure are provided in the reference cited. We find that maximum eclipse occurs roughly 38±1.9 seconds of time, on average, before the time of gravity maximum. If gravity is a propagating force, this three-body (Sun-Moon-Earth) test implies that gravity propagates at least twenty times faster than light.

Electromagnetic Analogies and Gravitational Radiation

Myth: Gravity from an accelerating source experiences light-time delay.

In electromagnetism, it is said that moving charges anticipate each other's linear motion, but not acceleration, and that acceleration causes the emission of photons. If gravity behaved in an analogous way, moving masses would anticipate each other's linear motion, but not acceleration, and accelerating masses would emit gravitational radiation. Indeed, the orbit of binary pulsar PSR1913+16 is observed to slowly decay at a rate close to that predicted by GR from the emission of gravitational radiation. Could that be evidence for changes in gravity propagating at light speed?

First, we will calculate the acceleration predicted for any two stars if each star responds to the linearly extrapolated retarded position and velocity, but not acceleration, of its companion over one light time between the stars. This would be consistent with the electromagnetic analogy. In Figure 3, we will consider the orbit of component A relative to component B during the light time between the two stars. We will then consider three positions of component A: its true, instantaneous position, A_t, its retarded position one light time ago, A_r, and its linearly extrapolated position one light time ahead from its retarded position, A_e. As before, let the product of the gravitational constant and the total system mass be μ, and the radius of A's circular orbit around B be a. Also let the speed of light be c, and A's orbital period be P. Finally, θ is the angle at B through which A moves during the light time a/c, and ψ is the angle at B between A_e and A_t. By construction, the linear distance from A_r to A_e is equal to the length of the arc from A_r to A_t, and both are equal to $a\theta$.

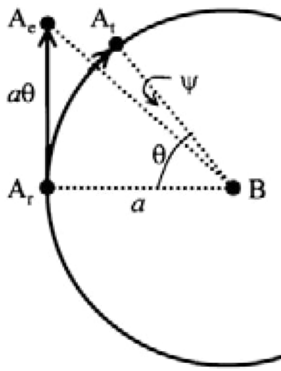


Figure 3. Comparison of a star's true position, A_t with its linearly extrapolated retarded position, A_e.

The difference in the distance of A_e and A_t from B causes only small, non-cumulative effects on the orbit. However, the angle ψ causes the extrapolated retarded position to feel a transverse force component that continually increases the orbital period P. From the triangles in the figure we see that $\psi = \theta - \tan^{-1}(a\theta/a)$. Since θ is normally a very small angle, we can expand the arctangent into a series and retain only significant terms. The result is $\psi \approx 1/3\theta^3$. However, θ is $2\pi/P$ times the light time, or $2\pi a/cP$. So the transverse perturbing acceleration B, which is ψ times the radial orbital acceleration μ/a^2, can be found from $B = 8/3\mu a(\pi/cP)^3$. Finally, from Danby,[2] and with some minor change of variables and simplification, we arrive at:

$$\dot{P} = 16\pi^4 (a/cP)^3 \tag{2}$$

Now we are ready to compare this prediction for binary pulsars; PSR1913+16 and PSR1534+12 with the measured values of $\dot{P}$ in the two best-observed cases. Orbital quantities are taken from Taylor[14] (see Table 1). The period change rate for PSR1534+12 is not yet seen, so the table shows the observational error of the measurement. At a glance, we see there is no possible match. The predicted period changes that would result if gravity propagated at the speed of light in a manner analogous to electromagnetic forces are orders of magnitude larger than the observed period changes. For PSR1913+16, they have the opposite sign as well. From PSR1534+12, we can set a lower limit to the speed of gravity as an electromagnetic-type propagating force: 2800*c*.

Table 1. Observed and predicted period change rate for two binary pulsars.

	PSR1913+16	PSR1534+12
a/c (sec)	2.342	3.729
P (sec)	27,907	36,352
$\dot{P}$ -observed	-2.42×10^{-12}	$\pm 0.6 \times 10^{-12}$
$\dot{P}$ -predicted	$+921 \times 10^{-12}$	$+1682 \times 10^{-12}$

We could have seen the essence of this result at the outset. Binary pulsars decay as they radiate away angular momentum, presumably in the form of gravitational radiation. However, a finite speed of propagation of gravitational force must add angular momentum to orbits. This is because the retarded position of any source of gravity must lie in the same direction relative to its true position as the tangential motion of the target body. Therefore, any delay in gravity will always pull the target in a direction that will increase its instantaneous orbital speed—the opposite of the effect of gravitational radiation.

In concluding this section, we should also note that even in the solar system, the Sun moves around the barycenter in a path that often takes the barycenter a million kilometers or so from the Sun. So the idea that the Sun's field can be treated as "static" and unchanging is not a good approximation even for our own planetary system. The Sun's motion during the light time to the planets is appreciable, yet its gravity field is continually updated without apparent delay.

Myth: Gravitational waves contribute to gravitational force.

Few subjects in physics are in such a state of confusion as is the subject of gravitational waves. Normally, this term is synonymous with gravitational radiation, a hypothetical, ultra-weak disturbance of space-time induced by a certain type of asymmetric change in the distribution of matter called a quadrupole moment. It is supposed to be analogous to accelerating charges emitting photons. This form of radiation is predicted by GR. The acceleration of binary pulsar PSR1913+16 is said to be in accord with the predicted amount of gravitational radiation, and therefore to provide an indirect confirmation of the prediction. However, attempts to detect gravitational waves in the laboratory from any source have yet to yield events that have convinced a consensus of their reality. The LIGO experiment is being designed to provide definitive detections, assuming these waves exist.

When gravitational waves were predicted, it was natural to associate them with supernova explosions, since no known event in nature redistributes mass in space more rapidly. However, the explosion must be asymmetric to produce gravitational waves. Because the gravitational field of the supernova is changing rapidly during the explosion, it is natural to associate the production of gravitational waves with changes in gravitational fields. So far, so good.

However, many physicists do more than associate the two concepts, and consider that changes in gravitational fields *are* gravitational waves. The heart of this confusion is illustrated by the following passage: "Suppose that a man, standing on the earth, holds in his hand a heavy club. At first the club hangs down toward the ground, but at a certain moment the man raises it quickly over his head. Any theory of gravitation recognizes that the club produces a gravitational field, however minute it may be, and that the action of the man changes that field, not only in his neighborhood, but throughout the whole universe. According to Newtonian theory, the effect is instantaneously felt on the moon, on the sun and in every remote nebula. Since we are not concerned with Newtonian theory, we do not have to discuss the absurdity of this. As relativists, familiar with the idea that no causal effect can travel faster than light, . . . we would guess that the change in the gravitational field of the moving club travels out into space with the speed of light. And we would call this moving disturbance a gravitational wave. Thus, on a very general basis, we must regard the physical existence of gravitational waves, so understood, as self-evident."[13]

The sudden displacement of the club may cause a disturbance of space-time, which would be a form of gravitational radiation. Separately, if gravitation is itself some sort of wave phenomenon, changes in gravitational fields will propagate away from a source as waves. Now there is no doubt that changes in gravitational fields exist, or that they can be detected in the laboratory. Therefore, this phenomenon cannot be the same thing as gravitational radiation, since the latter has not yet been reliably detected, and its existence still remains unverified. However, both phenomena are called "gravitational waves" without further distinction. For the former type, we must look to ultra-small accelerations of distant, massive pulsars for some hint of their existence. For the latter type, we see indirect evidence of changes in the gravitational fields of Sun and Moon every day in the tides, or can measure them directly with a gravimeter. We can even measure gravitational field changes using small masses in a purely laboratory setting.

The consequences of this distinction become clearer when we are careful to distinguish sources and targets of gravity. Ordinary gravitational acceleration of a target results from some form of communication from a source of gravity that may or may not be carried from source to target in wave form. Separately, the acceleration of a target body must change the nearby space-time, and such changes seem likely to be propagated outward in wave form away from the target. If possible waves associated with sources of gravity (those that may induce acceleration in other bodies), and other possible waves induced by targets of gravity (those that result from acceleration), are not distinguished, we are certain to have massive confusion over the meaning of the very concept of "the speed of gravity."

In a binary pulsar, where both masses are comparable, both stars may emit gravitational radiation. But each would do so as a consequence of its acceleration induced by the other, not as a consequence of its own gravity. Moreover, as we noted earlier, gravitational waves in the sense of gravitational radiation cause orbiting bodies to lose angular momentum, whereas gravitational aberration that must accompany any finite speed of propagation of gravity from a source to a target would cause orbits to gain angular momentum.

Therefore, it seems fairly certain that if gravitational radiation exists, its waves will propagate at the speed of light. In what way this type of disturbance of space-time may differ from very long wavelength electromagnetic disturbances of space-time, if indeed it does differ, remains to be seen.

By contrast, the speed of propagation of gravitational fields and of changes in those fields, whatever the nature of the propagating agents, are different matters, and pose a question we hope to answer in this paper.

Space-Time Curvature and Retarded Potentials

Is gravity caused by a curvature of space and time?

A common way to explain why gravity can appear to act instantaneously, yet still propagate with a delay, is the rubber sheet analogy. A large mass sitting on a rubber sheet would make a large indentation, and that indentation would induce smaller nearby masses to roll toward the indentation. This is an analogy for curved space-time, which is likewise supposed to be the cause of bodies accelerating toward large masses. The reasoning in the analogy further suggests that target bodies simply respond instantly to the local curvature of the underlying space-time medium (like the rubber sheet). Therefore, any delay associated with altering that local curvature would not produce aberration, and the target body would appear to respond instantaneously to the source unless the source suddenly changed its motion.

The rubber sheet analogy is represented as a way of visualizing why bodies attract one another. However, in that regard, it is highly defective. A target body sitting on the side of an indentation would stay in place, with no tendency to roll downhill, unless there were already a force such as gravity underneath the rubber sheet pulling everything downhill. And this failure of the analogy helps us identify the precise problem with the curved space-time description of gravity—the lack of causality. Without consideration of why a target body is induced to accelerate through space, and how quickly it receives updates of information about how to accelerate through space, neither the space-time curvature explanation nor the rubber sheet analogy can help us understand why gravity appears to act so much faster than light.

Moreover, contrary to what the rubber sheet analogy implies, an orbiting body such as a spacecraft orbiting the Earth is not following the curvature of space near the Earth. As we remarked earlier, two spacecraft some distance apart in the same orbit could stretch a tether between them and pull it taut, thereby describing a straight line through space different from their orbital path. In more mathematical terms, the supposed curvature of space-time produced by a gravitational field is an effect proportional to the local gravitational potential ϕ, the variable part of which is in turn proportional to v^2/c^2, where v is orbital speed. Yet, orbital curvature through space, like stellar aberration, is proportional to v/c, a much larger effect. For example, for the Earth orbiting the Sun, v/c is of order 10^{-4}, and v^2/c^2 is of order 10^{-8}. So we see that almost all of the acceleration of bodies through space is not a consequence of the curvature of space. In the GR explanation, the acceleration through space is due to the curvature of "space-time," a mathematical entity not to be confused with the combined separate concepts of space and time.

While relativists have always been partial to the curved space-time explanation of gravity, it is not an essential feature of GR. Eddington[3] was already aware of the mostly equivalent "refracting medium" explanation for GR features, which retains Euclidean space and time in the same mathematical formalism. In essence, the bending of light, gravitational red shift, Mercury perihelion advance, and radar time delay can all be consequences of electromagnetic wave motion through an underlying refracting medium that is made denser in proportion to the nearness of a source of gravity.[15,16] And it is now known that even ordinary matter has certain electromagnetic wave-like characteristics. The principal objection to this conceptually simpler refraction interpretation of GR is that a faster-than-light propagation speed for gravity itself is required. In the context of this paper, that cannot be considered as a fatal objection.

Lastly, we note experimental evidence from neutron interferometers that purports to demonstrate a failure of the geometric weak equivalence principle, that gravity is due to a curvature of space-time.[5] This experiment confirmed the strong equivalence principle—local equivalence of a uniform acceleration and a gravitational field—but its results are incompatible with the geometrical weak equivalence principle because interference effects in quantum mechanics depend on the mass. This is because the wave nature of the neutron depends on the momentum of the neutron, which is mass times velocity. So all phase-dependent phenomena depend on the mass through the wavelength, a feature intrinsic to quantum mechanics.

Since the experiment confirms the applicability of quantum mechanics even in the presence of gravity, including this non-geometrical mass dependence, the experiment seems to be a step in the undermining of the purely geometrical point of view, and "tends to bother theorists who prefer to think of gravity as being intrinsically related to geometry," according to the authors.

Does GR really reduce to Newtonian gravity in low-velocity, weak-field limit?

As we have already noted, Newtonian gravity propagates with unconditionally infinite speed. How, then, can GR reduce to Newtonian gravity in the weak-field, low-velocity limit? The answer is that conservation of angular momentum is implicit in the assumptions on which GR rests. However, as we have already seen, finite propagation speeds and conservation of angular momentum are incompatible. Therefore, GR was forced to claim that gravity is not a force that propagates in any classical sense, and that aberration does not apply.

In practice, this suppression of aberration is done through so-called "retarded potentials." In electromagnetism, these are called "Lienard-Wiechert potentials." For examples of the use of retarded potentials, see Misner *et al.*[11] or Feymnan.[4] Suppose we let $\phi(\vec{x},t)$ be the gravitational potential at a field point $\vec{x}$ and time t, G be the gravitational constant, dV be an element of volume in the source of the potential, $\vec{X} = (X, Y, Z)$ be the coordinates of that volume element in the source, $\rho(\vec{X},T)$ be the matter density at point $\vec{X}$ and time T, $\vec{r} = \vec{x} - \vec{X}, r = |\vec{r}|$ be the distance from the source volume element at time T to the field point at time t, and $\vec{v}$ be the relative velocity between the field point and the source. Then two different forms of retarded potentials in common use for gravitation are these:

$$\phi(\vec{x},t) - G\iiint \frac{\rho(\vec{X},t-r/c)}{r}\,dX\,dY\,dZ \tag{3}$$

$$\phi(\vec{x},t) \tag{4}$$

In *(3)*, we have used $T = t - r/c$ as the retarded time. Then the triple integral evaluates the density one light time ago in place of the present density, as might be useful if a non-spherically symmetric source body were rotating. In (4), the mutual distance is taken to depend on the scalar distance of the source one light time ago.

However, in neither form of retarded potential is any consideration given to the transverse motion between source and target during the light time, *i.e.*, the aberration. Ignoring aberration is logically equivalent to adopting an infinite propagation speed for gravitational force. That point is glossed over by emphasizing that the density distribution or the mutual distance is being taken at its retarded position, as if a finite propagation speed for gravity were being adopted. Nevertheless, the only practical consequence of a finite propagation speed that matters in most applications is missing from these potentials. And that clever trick then allows a theory with "gravity propagating at the speed of light" to be equivalent to a theory with infinite propagation speed in the weak-field, low velocity limit.

> *Self-introduction of a first-timer in the sci.physics.relativity newsgroup: "Let me start this out with the standard disclaimer... I am an idiot, I know almost nothing, I haven't taken calculus, I don't work for NASA, and I am one-quarter Bulgarian sheep dog. With that out of the way, I have several stupid questions. . ." Alex Wagner*

In short, both GR and Newtonian gravity use infinite propagation speeds with aberration equal to zero. In Newton's laws, that fact is explicitly recognized even though aberration and delay terms do not appear because of an infinity in their denominator. In GR, much effort has been expended in disguising the continued absence of the same delay terms by including retardation effects in ways that are presently not observable and ignoring aberration. Every physicist and physics student should be at least annoyed at having been tricked by this sleight of hand, and should demand that the neglect of aberration be clearly justified by those who propose to do so.

Does a Gravitational Field Continuously Regenerate, Or Is It "Frozen?"

In attempts to describe how GR can affect distant bodies seemingly without delay, relativists often speak of the field of a body as if it were a rigid extension of the body itself. If such a "static" field has no moving parts, it then would have no need of a propagation speed unless something changes. The objection to this picture is that it is acausal. Somehow, momentum is transferred from a source body to a target body. It seems impossible to conceive of a static field with literally no moving parts as capable of transferring momentum. This is the dilemma of the "rubber sheet" analogy again. Just because a rubber sheet or space-time is curved, why should a stationary target body on the slope of such a curve begin moving toward the source? What is the source of the momentum change?

To retain causality, we must distinguish two distinct meanings of the term "static." One meaning is unchanging in the sense of no moving parts. The other meaning is sameness from moment to moment by continual replacement of all moving parts. We can visualize this difference by thinking of a waterfall. A frozen waterfall is static in the first sense, and a flowing waterfall is static in the second sense. Both are essentially the same at every moment, yet the latter has moving parts capable of transferring momentum, and is made of entities that propagate.

As this applies to gravitational fields for a fixed source, if the field were static in the first sense, there would be no need of aberration, but also no apparent causality link between source and target. If the field were static in the second sense, then the propagation speed of the entities carrying momentum would give rise to aberration; and the observed absence of aberration demands a propagation speed far greater than light speed.

So are gravitational fields for a rigid, stationary source frozen, or continually regenerated? Causality seems to require the latter. If such fields are frozen, then what is the mechanism for updating them as the source moves, even linearly? Even a "rigid" bar pushed at one end would not move at the other end until a pressure wave had propagated its entire length. Moreover, we seem to need two mechanisms—one to curve space-time when a mass approaches, and another to unbend it when the mass recedes. This is because, once a curve is "frozen" into space-time, it will not necessarily "melt" back to its original condition when the cause is removed. Yet, there is no available cause for either process to result from a field with no moving parts.

We can also deduce the consequences for a source in continual acceleration, such as the Sun in our solar system. The Sun's path around the solar system barycenter induced by planetary perturbations causes excursions of over a million kilometers, and the barycenter is sometimes outside the physical body of the Sun. So the Sun's field must be continually updated at all distances to infinity. Surely, this updating requires the propagation of causal agents from the source. And since the source is continually accelerating, the regeneration of the distant field must likewise be a continuous process, requiring propagation. However, propagation involves delays, and even in the solar system, we have observationally ruled out delays as great as light speed propagation would produce. For example, the solar eclipse experiment is sensitive to delays in the continual updating of the Earth's field by the Sun as they both affect the Moon, and update speeds of at least 20 *c* are required.

The binary pulsar experiment provides another, more direct demonstration that even changes in gravitational fields must propagate faster than light. Ultimately, GR proposes that such changes appear to act instantaneously in the "near field," but eventually show their true, light-speed-delayed character in the "far field," which is conveniently beyond our present ability to observe. The necessity of this dual behavior is to prevent the logical need for changes to continue to appear to act instantaneously at ever increasing distances, even to infinity.

However, this only prevents certain types of paradoxes from arising. When the subject of "black holes" first comes up in physics classes, a frequently asked question is: "If nothing can escape the event horizon because nothing can propagate faster than light, how does gravity get out of a black hole?" The answer usually provided is that the field around a black hole was frozen into the surrounding space-time prior to the

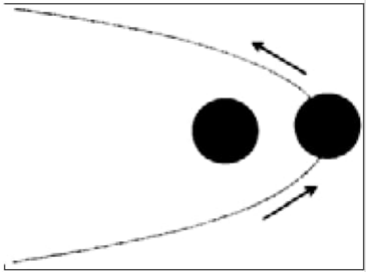


Figure 4. How can binary black holes update their external fields as they interact, when the masses are hidden behind event horizons?

collapse of the parent star behind an event horizon, and has remained in that state ever since. By implication, there is no need for continual regeneration of the external field by causal agents from the source.

However, let us suppose we have a binary black hole, with the two collapsed stars in elliptical orbits around one another. Then each field must be continually updated by a changing contribution from the orbiting field of the other. How does each field know what it is supposed to do if it is no longer in communication with its source mass hidden behind an event horizon? If the curvature of space-time at a point near black hole A becomes zero because black hole B is equally distant, what makes it non-zero again once black hole B recedes?

Indeed, if each source mass is forced to accelerate, why should each field point with a certain curvature undergo exactly the same acceleration as the source, making the whole field—to infinity?—appear frozen rigidly to the parent black hole? Perturbations by the other star are different at every different field point, so each such space-time field point should experience a different acceleration. With no communication, how can the whole system remain intact and coherent?

We conclude that the concept of frozen gravitational fields is acausal and paradoxical. Gravitational fields must continually regenerate, like a flowing waterfall. In doing so, they must consist of entities that propagate. And the speed of propagation of those entities must greatly exceed the speed of light.

Conclusion: The Speed of Gravity is ≥2x10^{10}*c*

We conclude that gravitational fields, even "static" ones, continually regenerate through entities that must propagate at some very high speed, v_g. We call this the speed of gravity. Equation (1) then tells us how orbits will expand in response to this large but finite propagation speed, since the field itself, and not merely changes in the field, will transfer momentum to orbiting target bodies. Rewriting Equation (1) in a form suitable for comparisons with observations, we derive:

$$\frac{\dot{P}}{P} = \frac{6\pi}{P}\frac{v}{c}\left[\frac{c}{v_g}\right] \qquad (5)$$

For the Earth's orbit, $P = 1$ year, $v/c = 10^{-4}$, and we take as an upper limit to $\dot{P}$/P the value 2.4x 10^{-12}/year (derived from $\dot{G}/G$) in solutions using radar ranging and spacecraft data.[12] Substituting these values, we get from Earth-orbit data that $v_g \geq 10^9 c$.

Using the same equation with binary pulsar PSR1534+12 and the parameters in Table 1, we can place the most stringent limit yet from the observed uncertainty in $\dot{P}$: $v_g \geq 2\text{x}10^{10}c$.

A direct experimental verification in the laboratory that gravity propagates faster than light may now be possible. The protocol and preliminary results were reported in Walker's paper.[18]

It might be tempting to conclude that the speed of gravity is infinite. But these limits on v_g are still a long way from infinite velocity, and Newton's statement, quoted at the beginning of this paper, still seems applicable. Infinite speeds, too, are acausal.

Consistency with Special Relativity

Einstein special relativity (SR) is able to prove based on its premises that nothing can propagate faster than the speed of light in forward time. Is our result for the speed of gravity an experimental falsification of SR? The correct answer must be a qualified "yes and no." Strictly, the minor new interpretation of SR needed for consistency with our result is no more a falsification of SR than GR was a falsification of Newtonian gravity. In both cases, the earlier theory was incomplete rather than wrong. We will now examine exactly what must change about SR for full consistency with all existing experimental evidence and this new result as well.

A brief overview of the history of relativity will provide useful background for this section, since everything proposed here has been proposed before. The "principle of relativity," that the laws of physics should be the same as viewed from any inertial frame, dates to the 19th century, well before it was popularized by H. Poincare. The well known "Lorentz transformations" embody that principle, but were not original when Lorentz adopted them for his own theory of relativity, first published in 1904 in an "aether" context. Einstein's main contribution with his famous 1905 paper, then, was the addition of a second postulate, that the speed of light will be locally the same for all observers regardless of their own state of motion. This did away with the need for an aether, or more generally, with a preferred frame of reference.

The ensuing years saw much discussion of whether nature was more like Einstein's SR or Lorentzian relativity (LR). The experiments relevant to testing relativity are listed in Table 2. The discovery of Fresnel drag had seemed at first to demand the existence of an aether, but relativists eventually found ways to explain it using SR, too. The Airy water-filled telescope experiment showed that the aberration of starlight was unchanged by passing through a water medium even though that medium slowed the speed of light by about 30%. This too seemed to favor the existence of a preferred frame because the local speed of light did not affect aberration, showing that aberration was determined outside the telescope rather than by the conditions most local to the observer. However, Einstein supporters could also explain this result using SR, albeit with somewhat more complexity.

The Michelson-Morley experiment is the first—and only—observation that seemed to strongly favor SR over LR, although Michelson himself never accepted that. The expected aether-drift speed did not put in an appearance in the test results, and the speed of light did indeed seem to be the same in all directions, as SR postulated, even though the observer was obviously moving at high speed in some direction through

Table 2. Independent experiments bearing on special relativity.

Experiment	Description	Year
Bradley	Discovery of aberration of light	1728
Fresnel	Light suffers drag from the local medium	1817
Airy	Aberration is independent of the local medium	1871
Michelson-Morley	Speed of light is independent of Earth's orbital motion	1881
De Sitter	Speed of light is independent of speed of source	1913
Sagnac	Speed of light depends on speed of a rotating platform	1913
Kennedy-Thorndike	Measured time as well as length is affected by motion	1932
Ives-Stilwell	Ions radiate at frequencies affected by their motion	1941
Frisch-Smith	Radioactive decay of mesons is slowed by their motion	1963
Hafele-Keating	Atomic clock changes depend on Earth's rotation	1982
GPS	(Various -- see text)	1997

space. It was not until the last decade that serious consideration was given to the possibility that the local gravity field may always constitute a preferred frame. This idea was popularized in Beckmann[1] and then widely discussed in the journals *Galilean Electrodynamics* (www.msx2.phajhu.edu/~dring/gehtmls/gehome.html) and *Apeiron* (www.redshift.vif.com/default.htm), and occasionally in the *Meta Research Bulletin* (www.metaresearch.org).

It is now well-established that LR is fully compatible with the Michelson-Morley experiment, and in general with the expectation that the speed of light will seem to be the same even when the observer is moving, provided that certain conditions are met, although not under all circumstances. That the speed of light is independent of the speed of its source is unremarkable, since that is a property of all wave motion. However, being independent of the speed of the observer is special. Choosing to synchronize clocks using the Einstein convention automatically makes one-way speed of light independent of the speed of the observer because that assumption is built into the Einstein synchronization method. If some other convention were used to synchronize clocks, such as synchronizing them to an underlying common inertial frame—as is done for the Global Positioning System satellites, or when astronomers synchronize phenomena to a barycentric frame using time provided by distant pulsars—then the one-way speed of light would be different in each direction when measured by observers moving with respect to that special frame. The round-trip speed of light uses a single clock to measure elapsed time, and so does not depend on synchronization. But if the rate of an ordinary clock is affected by its speed in a Lorentzian way, which we now know to be the case, then the measured speed of light will appear to be an invariant in all directions. Using a clock whose rate is not affected by its translational speed, for example pulses in the strength of the gravitational field from a compact, massive binary star, would apparently allow the speed of the observer relative to the local mean gravity field to be detected.

Following the publication of Einstein's SR paper, two new experimental results were published in 1913, both favoring LR over SR. Indeed, Sagnac claimed a falsification of SR on the grounds that the local speed of light was affected by observer velocity if the observer was attached to a rotating platform. He showed that the Michelson-Morley experiment performed in such a rotating frame did show fringe shifts, and concluded that, even if linear motion was relative, rotational motion was absolute. DeSitter noted that stellar aberration was the same for both components of distant binary stars, even though the relative velocity of each with respect to the observer was quite different. Therefore velocity in some special frame—we might now say velocity in the local gravity field relative to the distant gravity field—rather than relative velocity between source and observer determines aberration. Both of these experiments were blows to SR's contention that all motion was relative. Nonetheless, SR supporters came up with explanations of these phenomena too in an SR context, and these fairly non-trivial explanations are the subjects of textbooks on relativity today.

The Michelson-Gale experiment of 1925 involving the same Michelson as in the Michelson-Morley experiment again claimed a contradiction of SR—a theory that Michelson never found acceptable. History has concluded that this experiment is essentially another demonstration of the Sagnac effect and no longer cites it as a significant independent experiment; so it is omitted from our table. Ives and Stilwell[8] drew conclusions similar to those of Michelson, and specifically argued that their own experiment confirmed LR—which they called the Larmor-Lorentz theory—over SR. Yet today, it is simply added to the list of SR-confirming experiments.

When the muon lifetime experiments were performed in the 1960s, LR had been all but forgotten. Questions were raised briefly about whether the situation was reciprocal—whether high-speed muons would really see laboratory muons live longer. SR offered assurance that they would, but no test was then possible. By the time the Hafele-Keating experiment compared traveling atomic clocks sent around the world in opposite directions with a stay-at-home clock, an experiment later improved upon by C.O. Alley at the University of Maryland, it was no longer considered remarkable that the velocity effects on clocks had to be based on speeds in the underlying inertial frame instead of the relative velocities of the clocks.

Finally, the Global Positioning System (GPS) showed the remarkable fact that all atomic clocks on board orbiting satellites moving at high speeds in different directions could be simultaneously and continuously synchronized with each other and with all ground clocks. No "relativity of simultaneity" corrections, as required by SR, were needed. This too seemed initially to falsify SR. But on further inspection, continually changing synchronization corrections for each clock exist such that the predictions of SR are fulfilled for any local comoving frame. To avoid the embarrassment of that complexity, GPS analysis is now done exclusively in the Earth-centered inertial frame (the local gravity field). And the pre-launch adjustment of clock rates to compensate for relativistic effects then hides the fact that all orbiting satellite clocks would be seen to tick slower than ground clocks if not rate-compensated for their orbital motion, and that no reciprocity would exist when satellites view ground clocks.

Why then did SR win out over LR? Three circumstances conspired to make SR appear to be the better solution to describing nature in the early years of the 20th century: (1) Classical thinking about the aether almost always involved a universal field rather than a local field. No one took seriously that each local gravity field might serve as a preferred frame for local observers. Yet that now seems the case. (2) The wave nature of matter had not yet been discovered by deBroglie. Before that happened, there was no logical reason to expect that clocks based ultimately on atomic oscillations would have their rates affected by observer motion in the same way that the speed of light would be affected by observer motion, rendering observer motion undetectable in experiments. However, that also now seems to be true.[15] (3) The success of GR in predicting the light-bending effect at the 1918 solar eclipse gained great credibility for GR, and SR benefited from this success because it was widely believed that GR was based on SR. But, GR is usually implemented using a preferred frame closely coinciding with the local gravity field, with the consequence that only the features that SR and LR have in common were integrated into GR. The reciprocity of time dilation between two inertial frames, a key way in which SR differs from LR, plays no role in GR.

The principal differences between the two relativity theories stem from the equivalence of all inertial frames in SR, and the existence of a preferred frame in LR. Otherwise, SR's time dilation is equivalent to LR's clock slowing; SR's space contraction is equivalent to LR's meter-stick shrinkage; and SR's change in the momentum of moving bodies is equivalent to LR's. However, LR recognizes a "universal time" apart from the time kept by electromagnetic-based clocks affected by motion. And the law of addition of velocities between two frames, neither of which is the preferred frame, is different in LR than in SR. For a derivation of this law and the revised form of the Lorentz transformations for Lorentzian universal time, see Mansouri and Sexl.[10] For our purposes here, we simply note that the proof that nothing can propagate faster than the speed of light in forward time does not apply to LR.

Near the end of his career, Lorentz is quoted as having graciously conceded the contest: "My theory can obtain all the same results as special relativity, but perhaps not with a comparable simplicity." (private communication from C.O. Alley) Today, with hindsight, we might make a somewhat different assessment: "Special relativity can explain all the experimental results in Table 2 that Lorentzian relativity can, but perhaps not with a comparable simplicity." Even so, SR cannot explain the faster-than-light propagation of gravity, although LR readily can.

We conclude that the speed of gravity may provide the new insight physics has been awaiting to lead the way to unification of the fundamental forces. As shown in Van Flandern,[15,17] it may also be connected with the explanation of the dark matter problem in cosmology. Moreover, the modest switch from SR to LR may correct the "wrong turn" physics must have made to get into the dilemma presented by quantum mechanics, that there appears to be no "deep reality" to the world around us. Quantum phenomena that violate the locality criterion may now be welcomed into conventional physics.

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The author further thanks Jeffery Kooistra for his key role. His *Analog* article[7] flushed this subject to the forefront once again, and his inquiries to both Steve Carlip and to the author forced us to explain our positions in layman's language, and thereby diverted us from talking past one another. Discussions with colleagues too numerous to mention must likewise be acknowledged. But Jean-Pierre Vigier, in addition to several penetrating questions, encouraged the author to stop talking and start writing, promising a fair peer review process at the conclusion. Without such encouragement, this paper would certainly not have come into existence.

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About the Author

Tom received his Ph.D. in Astronomy from Yale University in 1969. He spent twenty years at the U.S. Naval Observatory, where he became the Chief of the Celestial Mechanics Branch. Tom has since formed a Washington, DC-based organization, Meta Research, to foster research into ideas not otherwise supported solely because they conflict with mainstream theories in astronomy. He is a research associate at the University of Maryland Physics Department, where he does research on the accuracy of the Global Positioning System. Tom has written *Dark Matter, Missing Planets and New Comets*, published by North Atlantic Books. He was the recipient of a prize from the Gravity Research Foundation; served on the Council of American Astronomical Society's Division on Dynamical Astronomy; taught astronomy at the University of South Florida and to Navy Department employees; and appeared on the "Project Universe" series that airs occasionally on public television.

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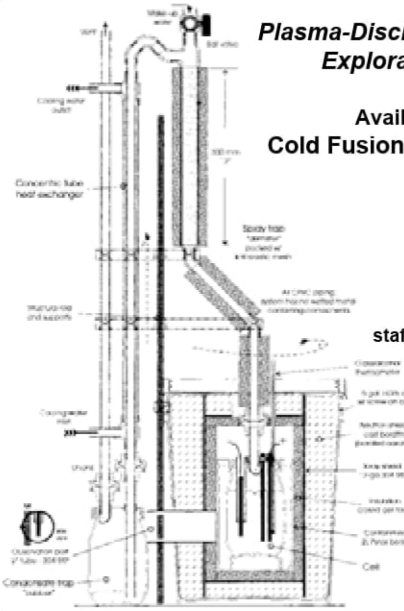

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The Photon Reactor: Producing Power By Burning Nuclear Waste

Paul M. Brown*

Summary

A linear accelerator, preferably of the monochromatic type, accelerates electrons which are directed onto a high Z target such as tungsten to generate gamma rays about 9 MeV, which are directed onto the fuel material such as U-238 which results in the (γ,f) reaction, thus releasing about 200 MeV. A reactor built according to this principle requiring an accelerator driven by 1 MW will develop about 20 MW of power. The reaction is not self-sustaining and stops when the beam is turned off. This accelerator driven reactor may be used to "burn up" spent fuel from fission reactors, if simply operated at 10 MeV. The photo-fission results in typical spent fuel waste products such as Cs^{137} and Sr^{90}, which undergo photodisintegration by the (γ,n) reaction resulting in short-lived or stable products. Chemical separation of the spent fuel isotopes is not necessary. Of course, more than one accelerator may be used to drive the reactor to higher power levels and speed up the burn up process. The fact that the reaction is not self-sustaining is a safety feature allowing immediate shutdown in the event of a problem.

Introduction

The nuclear fission of heavy elements following the absorption of electromagnetic radiation—photofission—was first predicted by Bohr and Wheeler[1] in their famous 1939 paper. Haxby, Shoupp, Stephens, and Wells (1941) were the first to produce fission with gamma rays.

A survey of the literature indicates that photonuclear reaction studies in actinide nuclei have been the pursuit of several laboratories during the last forty years, using several types of gamma sources. The main objective of these studies has been to obtain nuclear information at excitation energies in the region of the giant dipole resonance and in the region of low energy, near the photofission and photoneutron thresholds.

Bowman,[2] using a quasi-monochromatic photon beam obtained from the annihilation in flight of monochromatic positrons, was the first to observe the characteristic splitting of the giant dipole resonance and of a fissile nucleus into two components, a phenomenon observed for other permanently deformed nuclei as well.[4] However, they found that the photon-induced Γ_n/Γ_f ratio was strongly energy dependent, a result in complete disagreement with data obtained from neutron-induced fission, bremsstrahlung-induced fission, and charge-particle-induced fission.[3]

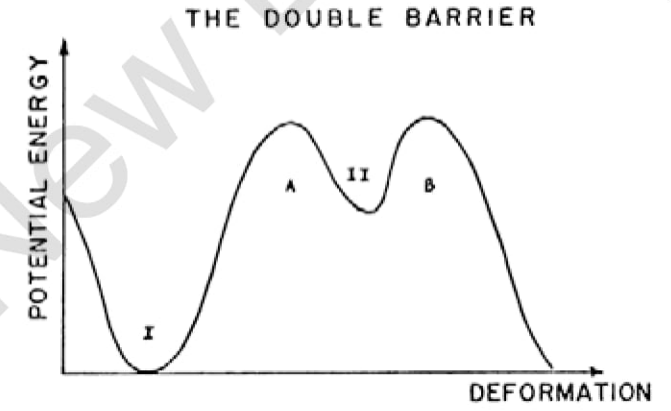

Figure 1. The characteristic double-humped curve typical of strongly deformed nuclei.

It is well-known that at neutron number N=90 a sharp change in the nuclear surface properties occurs. The resulting transition from equilibrium spherical to prolate nuclear shape leads to a change in the nuclear optical anisotropy and, consequently, in the shape of the giant resonance in the photoabsorption cross section as shown in Figure 1. Since many nuclear properties depend similarly on the proton and neutron numbers, it is reasonable to expect that analogous transition effects should be observed for Z=90 nuclei as well, which will lead to the evolution of their photoabsorption cross section shape.[5] This giant-resonance absorption is ascribed to the excitation of dipole vibrations of all the protons against all the neutrons in the nucleus, the protons and neutrons separately behaving as compressible fluids. This model makes some fairly simple predictions about the magnitude and A-dependence of the resonance that are quite well borne out by the experimental data: the integrated cross sections under the resonance peaks are given to good approximation by 0.06NZ/A MeV b, and the peak energies can be approximately represented by $aA^{-1/3}$.

The energy of the dipole resonance is so low that mostly rather simple processes—such as (γ,n) and photofission reactions—take place in the giant-resonance region. The competition between these processes is governed by the usual statistical considerations of compound-nucleus de-excitation, so that neutron emission usually dominates.

The characteristics of the giant dipole resonance for the actinide nuclei are of particular interest. For such high-Z, high-Coulomb barrier nuclei, the total photon-absorption cross section is equal to the sum of the photoneutron and photofission cross sections. The total photoneutron cross section is the sum of the following reaction cross sections,

$$\sigma(\gamma, n_{tot}) = \sigma(\gamma,n) + 2\sigma(\gamma,2n) + \nu\sigma(\gamma,f) \qquad (1)$$

where ν is the neutron multiplicity of a fission event. The total neutron production cross section is then,

$$\sigma_{\gamma,N} = \sigma_{\gamma,n} + \nu\sigma_{\gamma,f}\,. \qquad (2)$$

The competition between neutron emission and fission may be expressed,

$$\Gamma_n/\Gamma_f(E) = \sigma(\gamma,n)/\sigma(\gamma,f)(E). \qquad (3)$$

The value for Γ_n/Γ_f decreases exponentially with the fissility of the nuclei.[6] The theoretical expression for Γ_n/Γ_f which explains this behavior for the neutron emission and fission competition is derived from the Constant Nuclear Temperature for the level density, and is expressed

$$\Gamma_n/\Gamma_f = 2\ TA^{2/3}/10\ \exp\{(Ef' - Bn')/T\} \qquad (4)$$

where (Ef′ - Bn′) are the effective thresholds for the respective photonuclear processes and T is the nuclear temperature.

The fact that more than one neutron is emitted per fission in the fission of such isotopes as Th^{232}, U^{233}, U^{235}, U^{238}, and Pu^{239} leads to the possibility of a chain reaction in a mass of fissionable material. Whether the chain reaction remains steady, builds up, or dies down depends upon the competition between the production of neutrons through fission and the loss of neutrons through a variety of processes such as non-fission capture of neutrons, primarily (n,γ) reactions in the system, and the leakage of neutrons through the surface of the system.

Energy is released at the rate of 200 MeV per fission of one atom or about 23 x 10^6 kw-hr per fission of one kilogram of U^{235}. The fission fragments carry off 82% of the energy in the form of kinetic energy. Prompt neutrons carry off another 2.5%, prompt gammas carry off 3.5%, beta decay accounts for 4%, delayed gammas account for 3%, and neutrinos carry off the remaining 5%. The neutrinos and their energy are lost, since the probability of interaction with neutrinos is so small. Some fission also occurs as a fast neutron strikes a U^{238} atom. Also, as the fuel is burned plutonium is produced, and by the end of a fuel cycle—eighteen months of operation—35% of the energy is actually coming from the fission of Pu^{239} atoms. About 80% of the neutron absorption in U^{235} results in fission; the other 20% are (n,γ) reactions.

Once a fission chain reaction is started, the *effective multiplication factor* k_e will determine whether the chain reaction will continue at a steady rate, increase, or decrease. The effective multiplication factor is defined as *the ratio of the rate of production of neutrons*, P, *to the combined rate of absorption*, A, *and the rate of leakage*, L, *of neutrons*, or k_e=P/A+L. The term *absorption* includes all types of absorption, such as those which produce fission and those which produce (n,γ) processes in the material of the reactor. The fission chain reaction will be critical or steady when k_e=1, it will be building up or *supercritical* when k_e>1, and it will be dying down or *subcritical* when k_e <1.

If F is the rate at which fission processes occur, and if ν is the average number of neutrons emitted per fission, then P=νF. Then we may write k_e =P/A+L as k_e=νF/A+L from which we get

$$k_e = \nu(F/A)\ [1/1+(L/A)]. \qquad (5)$$

The ratio F/A depends upon the amount of fissionable and nonfissionable material and on their cross sections for fission and neutron capture. The ratio L/A depends upon the ability of the reactor to contain and absorb neutrons before they can escape through the surface. As the size of a reactor decreases, the rate of neutron leakage through the surface increases, and the rate of neutron absorption decreases, so that L/A increases and approaches infinity, and hence in the limit k_e approaches zero. As the size of the reactor increases, L/A decreases toward zero, and k_e increases toward the limiting value νF/A. Hence if the composition of the reactor is such that νF/A>1, then there is some size of this reactor for which k_e=1; for this size, the reactor is critical. This size is called the *critical size* and the mass of fissionable material at this size is called the *critical mass*. The region containing the fissionable material is called the *reactor core*. The core may be surrounded by nonfissionable material capable of reflecting neutrons back into the core; in such a case, both the critical size and the critical mass are reduced. On the other hand, if there is an insufficient amount of fissionable material or an excess of absorbing material in the reactor core so that νF/A<1, then there is no size for which a steady chain reaction can occur, irrespective of whether or not a reflector is used. Pure natural uranium, no matter how large the amount, cannot support a chain reaction, that is νF/A<1. Titterton (1950) found that the average kinetic energy released in the photofission of Th^{232} is about 0.8 of that released in the slow-neutron fission of U^{235} or about 160 MeV.

Table 1. Thermal Neutron Cross Sections for Uranium

Process	Cross Section (Barns)		
	U^{235}	U^{238}	U^{natural}
Fission	549	0	3.92
n-Capture	101	2.80	3.5
Scattering	8.2	8.2	8.2

Table 2. Fission Threshold Energy of Select Isotopes

NUCLIDE	PHOTOFISSION THRESHOLD (MeV)	NEUTRON-FISSION THRESHOLD (MeV)
Am^{241}	6.0	—
Am^{242}	—	6.4
Th^{232}	5.8	1.3
Np^{237}	5.6	0.4
Np^{238}	—	6.0
U^{233}	5.7	0.025
U^{234}	6.0	0.4
U^{235}	5.3	0.025
U^{236}	—	0.8
U^{237}	—	6.3
U^{238}	5.8	1.2
Pu^{239}	5.8	0.025

The Nuclear Waste Problem

A typical 1000 MWe PWR reactor operating at 75% capacity generates about twenty-one tons of spent fuel at a burn-up of 43 GWd/t. The twenty-one tons of spent fuel—contained inside 42 PWR fuel elements with a total volume of about 11 m^3—will have produced an electric energy of about 6.6 TWh (6.6 billion kWh). This same energy output corresponds to the burning of two million tons of coal in a conventional power plant giving rise to 120,000 tons of ashes, 5.4 million tons of CO$_2$, and 50,000 tons of SO$_2$.

Spent fuel consists of uranium which accounts for about 96% of the spent fuel removed from commercial nuclear reactors. In the case of light water reactors (the type most commonly used), the spent fuel contains 0.90% U^{235}, whereas natural uranium contains only 0.70% of this isotope. Plutonium constitutes about 1% of the weight of spent fuel. It is fissile, which means that it can be used as fuel in nuclear reactors. The minor actinides constitute about 0.1% of the weight of spent fuel. They consist of about 50% Np, 47% Am, and 3% Cm, which are very radiotoxic. The fission products—iodine, technetium, neodymium, zirconium, molybdenum, cerium, cesium, ruthenium, palladium, etc.—constitute about 2.9% of the weight of spent fuel.

The two fission products of principal concern, because of their substantial thermal impact on the repository as opposed to posing a health risk, are Sr^{90} and Cs^{137}. These two radionuclides are dominant contributors to the heat released by spent fuel at least for the first several decades. Cs^{137} is also a major source of penetrating radiation emitted by spent fuel. The two fission products of principal concern, because of their potential contribution to health risk, are Tc^{99} and I^{129}. They are of principal concern because they are long-lived, produced in significant amounts in the fission process, generally soluble under geologic conditions, and migrate relatively quickly under common ground water conditions.

The long-term toxicity of spent fuel is dominated by the actinides such as Np^{237}, U^{234}, U^{236}, and Pu^{239}, Pu^{240}, and Pu^{242}. The transmutation of long-lived nuclides in high level waste to stable or short-lived nuclides by stimulating nuclear reactions is a desirable alternative approach for the reduction of high level waste.

There are about three hundred different radioactive species generated by the operation of a nuclear reactor, primarily as a result of neutron capture and neutron-induced fission. The adverse impact of the various radionuclides varies because of the differences in the chemical behavior in the body of, and the radiation emitted by, the radionuclides. The risk focus of the radionuclides is related to waste disposal in a geologic repository. The most common release and exposure mechanisms from a repository involve ground water contacting the waste form followed by slow dissolution, transport of radionuclides to the accessible environment, distribution in the biosphere, and eventual uptake from food and water. Although hundreds of isotopes are present in spent fuel or wastes derived from them, only a few of them are important for disposal. These four isotopes—Cs^{137}, Sr^{90}, I^{129}, and Tc^{99}—are the primary focus of concern for light-water reactor spent fuel, *i.e.*, nuclear waste due to their excess heat, groundwater solubility, or health risk.

The management of spent fuel should ensure that the biosphere is protected under economically acceptable conditions without entailing unfavorable short-term consequences and the public must be convinced of the effectiveness of the methods. Since the spent fuel contains very long-lived radionuclides, some protection is required for at least one hundred thousand years. Two means are possible:

1. We can wait for the natural decay of the radioactive elements by isolating them physically from the biosphere by installing successive barriers at a suitable depth in the ground. This strategy is called deep geological disposal;
2. We can make use of nuclear reactions that will transmute the very long-lived wastes into less radioactive or shorter-lived products. This strategy is called transmutation.[7]

The problem with storing nuclear waste below ground is that there is no material that will outlast its radioactive contents and radioactive wastes continuously produce heat, hydrogen, and helium outgassing, as well as other labile products.

The nuclear industry with the federal government have spent more than $6 billion in development of the Yucca Mountain, Nevada, site where they plan to store 77,000 metric tons of high-level radioactive waste. A June 29, 1992 earthquake of 5.9 magnitude on the Richter scale caused $1 million in damage to a Department of Energy building six miles from the proposed Yucca Mountain site. Department of Energy scientists were rattled to discover that the epicenter of the quake was twelve miles from the proposed dump site.

In 1991, mining experts reported that a deep underground salt chamber (Waste Isolation Pilot Plant, or WIPP) in the New Mexico desert designated for the first United States tests of permanent radioactive waste disposal would probably collapse years before the tests could be completed. The WIPP facility was to start accepting waste in January 1999.

Table 3. Fissionabilities

Nucleus	Z^2/A	$\sigma_{int}(\gamma,F)/\sigma_{int}(\gamma,tot)$	Γ_n/Γ_f
Th^{232}	34.91	0.11	15
U^{238}	35.56	0.30	3.9
U^{236}	35.86	0.46	2.1
U^{235}	36.02	0.62	1.4
U^{234}	36.17	0.68	0.99
U^{233}	36.33	0.81	0.49
Np^{237}	36.49	0.60	0.68
Pu^{239}	36.97	0.74	0.62

There are some one hundred fourteen nuclear reactors in the United States and about four hundred commercial nuclear power plants in operation around the world including about one hundred twenty GWe nuclear electric capacity in Western Europe and forty-five GWe operational in the ex-USSR and Eastern European countries. In the United States alone, we have accumulated thirty-four thousand tons of nuclear waste. The current United States production rate of high-level waste—primarily spent fuel—is three thousand tons per year. The average commercial power plant puts sixty used fuel assemblies into "temporary" storage each year and is expected to do so until the year 2000 when the waste is to be transferred to the Department of Energy. This does not include low-level wastes such as gloves, filters, tools, clothing, etc. that come from nuclear power plants, research centers, and hospitals that use radioactive materials. There are about one hundred thousand United States facilities that use radioactive materials. They produce 1.6 million cubic feet of low-level waste each year.

Current projected costs of the United States Environmental Management Program are about $7.5 billion per year. Paper studies currently account for about 20% of the Environmental Restoration budget. According to the Baseline Environmental Management Report, the total clean-up cost of the nuclear weapons program is $230 billion over a seventy-five year period, including the $50 billion projected Hanford clean-up.

The Solution: Sub-critical Accelerator Driven Reactor

The photon reactor is a method and means for producing nuclear energy from heavy elements but not fissile elements. The reaction is not driven by the well-known self-sustained, chain-reaction of U^{235}, rather by an accelerator. The fuel for this type of accelerator driven reactor may be the spent fuel from fission reactors. The mechanism by which nuclear energy is released from non-fissile material is known as photofission, wherein a photon or gamma is introduced greater than the photofission threshold energy resulting in fission of the target nucleus. For instance, with U^{238} the threshold of photofission is about 6 MeV and results in fission of the U^{238} nucleus releasing about 200 MeV. Patents are currently pending.

A linear accelerator, preferably of the monochromatic type, accelerates electrons which are directed onto a high Z target, such as tungsten, to generate gamma rays of an energy about 10 MeV,

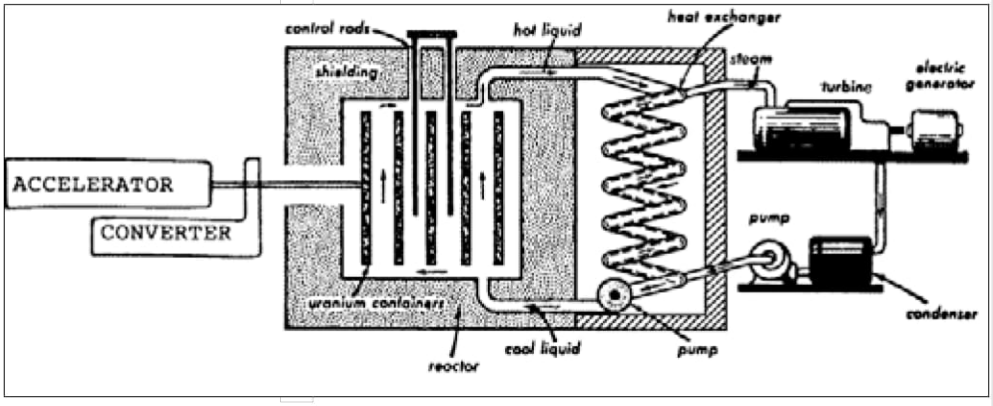

Figure 2. The accelerator driven reactor schematic.

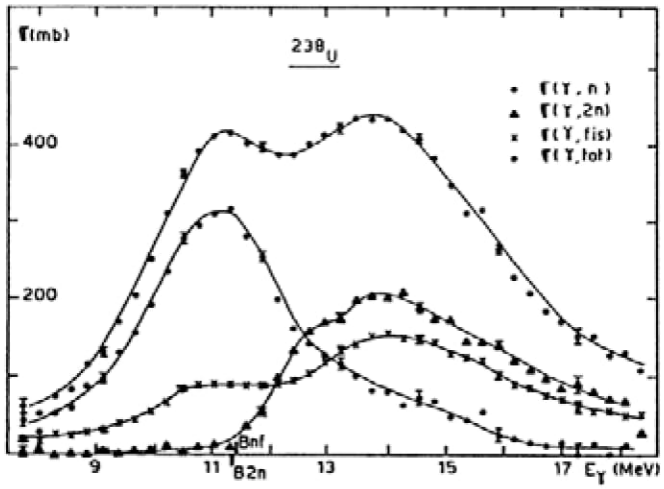

Figure 3. Partial and total photonuclear cross sections (γ,n), (γ,2n), (γ,f), and (γ,tot) for U^{238}.

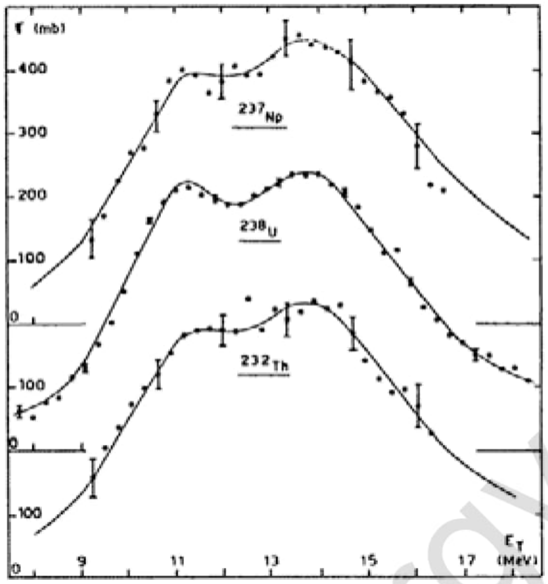

Figure 4. Total photonuclear cross sections for Th^{232}, U^{238}, and Np^{237}.

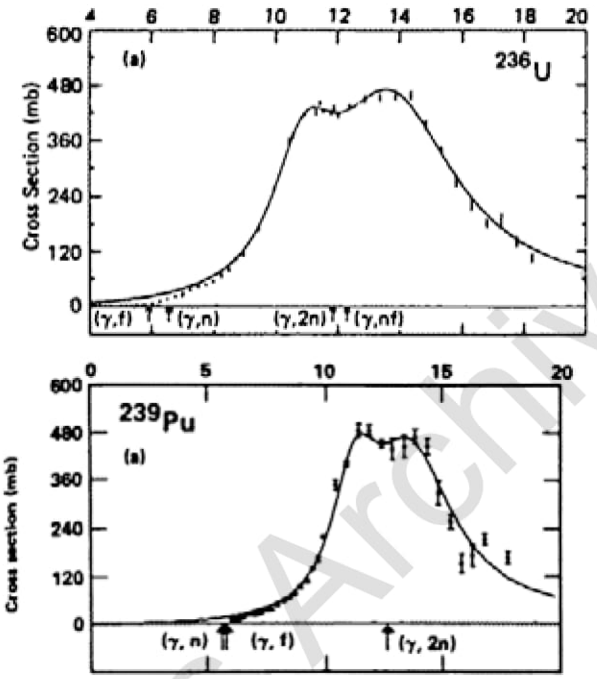

Figure 5. Total photonuclear cross section for U^{236} and Pu^{239}.

which are directed onto the fuel material such as U^{238}, resulting in the (γ,*f*) reaction, thus releasing about 200 MeV. A reactor, built according to this principle requiring an accelerator driven by 1 MW, will develop about 20 MW of power. The reaction is not self-sustaining and stops when the beam is turned off. This accelerator driven reactor may be used to "burn up" spent fuel from fission reactors if simply operated at 10 MeV. The photofission results in typical spent fuel waste products such as Cs^{137} and Sr^{90} which undergo photodisintegration by the (γ,n) reaction, resulting in short-lived or stable products. Chemical separations of the spent fuel isotopes are not necessary. Of course, more than one accelerator may be used to drive the reactor to higher power levels and speed up the burn up process. Ideally, four spaced accelerators would require about 4.8 MW of power to run, resulting in about 100 MW from the reactor.

Table 4. Relevant Reactions in Photon Driven Reactor

PhotoFission Reactions:

(γ,*f*): U^{238}, Th^{232}, Pa^{231}, Np^{237}, Np^{238}, Pa^{232}, Pu^{239}, Pu^{241}, Th^{227}, U^{231}, U^{233}, U^{235}.

Fission Reactions: (just one of many)

$${}_{92}U^{235} + {}_0n^1 \rightarrow {}_{38}Sr^{94} + {}_{54}Xe^{140} + 2\,{}_0n^1$$
$${}_{38}Sr^{94} \rightarrow {}_{39}Y^{94} + \beta^- \rightarrow {}_{40}Zr^{94} + \beta^- \text{ (stable)}$$
$${}_{54}Xe^{140} \rightarrow {}_{55}Cs^{140} + \beta^- \rightarrow {}_{56}Ba^{140} + \beta^- \rightarrow {}_{57}La^{140} + \beta^- \rightarrow {}_{58}Ce^{140} + \beta^- \textbf{ (stable)}$$

(γ,n) Enrichment:

$${}_{92}U^{236} + \gamma \rightarrow {}_{92}U^{235} + {}_0n^1$$
$${}_{92}U^{238} + \gamma \rightarrow {}_{92}U^{236} + 2\,{}_0n^1 + \gamma \rightarrow {}_{92}U^{235} + {}_0n^1$$
$${}_{95}Am^{241} + \gamma \rightarrow {}_{95}Am^{240} + {}_0n^1 + {}_{-1}e^0 \rightarrow {}_{94}Pu^{240}$$
$${}_{93}Np^{237} + \gamma \rightarrow {}_{93}Np^{235} + 2\,{}_0n^1 + {}_{-1}e^0 \rightarrow {}_{92}U^{235}$$
$${}_{92}U^{238} + \gamma \rightarrow {}_{92}U^{237} + {}_0n^1 + \gamma \rightarrow {}_{92}U^{236} + {}_0n^1 + \gamma \rightarrow {}_{92}U^{235} + {}_0n^1$$
$${}_{94}Pu^{238} + \gamma \rightarrow {}_{94}Pu^{236} + 2\,{}_0n^1 + \gamma \rightarrow {}_{94}Pu^{235} + {}_0n^1 + {}_{-1}e^0 \rightarrow {}_{93}Np^{235} + {}_{-1}e^0 \rightarrow {}_{92}U^{235}$$
$${}_{92}U^{238} + \gamma \rightarrow {}_{92}U^{237} + {}_0n^1 \rightarrow {}_{93}Np^{237} + \beta^- + \gamma \rightarrow {}_{93}Np^{235} + 2\,{}_0n^1 + {}_{-1}e^0 \rightarrow {}_{92}U^{235}$$
$${}_{94}Pu^{239} + \gamma \rightarrow {}_{94}Pu^{237} + 2\,{}_0n^1 + \gamma \rightarrow {}_{94}Pu^{235} + 2\,{}_0n^1 + {}_{-1}e^0 \rightarrow {}_{93}Np^{235} + {}_{-1}e^0 \rightarrow {}_{92}U^{235}$$
$${}_{94}Pu^{239} + \gamma \rightarrow {}_{92}U^{235} + \alpha$$

Neutron Enrichment:

$${}_{92}U^{234} + {}_0n^1 \rightarrow {}_{92}U^{235} + \gamma$$
$${}_{92}U^{238} + {}_0n^1 \rightarrow {}_{92}U^{239} + \gamma \rightarrow {}_{93}Np^{239} + \beta^- \rightarrow {}_{94}Pu^{239} + \beta^-$$
$${}_{90}Th^{232} + {}_0n^1 \rightarrow {}_{90}Th^{233} + \gamma \rightarrow {}_{91}Pa^{233} + \beta^- \rightarrow {}_{92}U^{233} + \beta^-$$

(γ,n) Neutralization:

$${}_1H^3 + \gamma \rightarrow {}_1H^2 + {}_0n^1 \text{ (stable)}$$
$${}_6C^{14} + \gamma \rightarrow {}_6C^{13} + {}_0n^1 \text{ (stable)}$$
$${}_{39}Y^{90} + \gamma \rightarrow {}_{39}Y^{89} + {}_0n^1 \text{ (stable)}$$
$${}_{28}Ni^{63} + \gamma \rightarrow {}_{28}Ni^{62} + {}_0n^1 \text{ (stable)}$$
$${}_{36}Kr^{85} + \gamma \rightarrow {}_{36}Kr^{84} + {}_0n^1 \text{ (stable)}$$
$${}_{27}Co^{60} + \gamma \rightarrow {}_{27}Co^{59} + {}_0n^1 \text{ (stable)}$$
$${}_{81}Tl^{204} + \gamma \rightarrow {}_{81}Tl^{203} + {}_0n^1 \text{ (stable)}$$
$${}_{38}Sr^{90} + \gamma \rightarrow {}_{38}Sr^{88} + 2\,{}_0n^1 \text{ (stable)}$$
$${}_{83}Bi^{210} + \gamma \rightarrow {}_{83}Bi^{209} + {}_0n^1 \text{ (stable)}$$
$${}_{56}Ba^{133} + \gamma \rightarrow {}_{56}Ba^{132} + {}_0n^1 \text{ (stable)}$$
$${}_{82}Pb^{210} + \gamma \rightarrow {}_{82}Pb^{208} + 2\,{}_0n^1 \text{ (stable)}$$
$${}_{38}Sr^{90} + \gamma \rightarrow {}_{38}Sr^{89} + {}_0n^1 \rightarrow {}_{39}Y^{89} + \beta^- \text{ (stable)}$$
$${}_{53}I^{129} + \gamma \rightarrow {}_{53}I^{128} + {}_0n^1 \rightarrow {}_{54}Xe^{128} + \beta^- \text{ (stable)}$$
$${}_{55}Cs^{137} + \gamma \rightarrow {}_{55}Cs^{136} + {}_0n^1 \rightarrow {}_{56}Ba^{136} + \beta^- \text{ (stable)}$$
$${}_{26}Fe^{60} + \gamma \rightarrow {}_{26}Fe^{59} + {}_0n^1 \rightarrow {}_{27}Co^{59} + \beta^- \text{ (stable)}$$
$${}_{82}Pb^{210} + \gamma \rightarrow {}_{82}Pb^{209} + {}_0n^1 \rightarrow {}_{83}Bi^{209} + \beta^- \text{ (stable)}$$

Reactions in the Accelerator Driven Reactor

It is important to note that although the reactor is sub-critical and driven by gamma rays, the neutrons produced still induce both fast and slow neutron fission just as in any conventional reactor. These neutron reactions result in additional energy output, thereby increasing the input/output ratio from 1/20, a value determined by the design.

Figure 3 shows the photonuclear cross sectional data obtained by Veyssiere[3] for U^{238}. Notice that the total photonuclear cross sections all have about the same peak cross section value. The maximum cross sections are all about 0.5b and all are about 6 MeV wide; this appears to hold true for all the actinides. The photoabsorption cross section falls sharply above the $(\gamma,2N)$ and (γ,nf) peaks, as is the case for essentially all medium and heavy nuclei as provided by the examples in Figure 4.

The thermal fissionable nuclides include Np^{238}, Pa^{232}, Pu^{239}, Pu^{241}, Th^{227}, U^{231}, U^{233}, and U^{235}. All those nuclides fissionable by thermal neutrons are, of course, also fissionable by fast neutrons. In addition, there are several nuclides such as U^{238}, Th^{232}, Pa^{231}, and Np^{237} which are fissionable by neutrons having energies of about 1 MeV.

Looking at Figures 3 through 5 we see that at 10 MeV the (γ,n) reaction is about three times the (γ,f) reaction. Table 4 lists several (γ,n) reactions that result in the neutralization or burn-up of the radioisotope resulting in stable, non-radioactive products. The reactions that occur within the accelerator driven reactor are too numerous to list but the most important reactions are shown in Table 4.[9]

In a (γ,n) reaction neither the γ-ray nor the neutron has a Coulomb barrier to surmount, so reaction sets in sharply as soon as the threshold energy is reached.[8]

For many fission products the neutron capture cross sections in a thermal spectrum can give substantial transmutation rates. The transmutation of Tc^{99} is characteristically much more effective in a thermal neutron spectrum, generally due to higher neutron capture cross section at lower energies.

The systematics of the giant dipole resonance, which characterizes the absorption of electromagnetic radiation by nuclei in the energy range from about 5 to 30 MeV, have been of interest since the discovery of the giant resonance itself. Over the years, the photoneutron cross sections for many nuclei have been measured with monoenergetic photons in numerous laboratories. All these data are presented in the Atomic Data Nuclear Data Tables. For most cases studied, the agreement is remarkably good.

The Accelerator

The high-energy X-ray machine requires a high-power, low-energy (10 MeV) electron linac to produce the gamma rays to drive the reactions in the reactor. Current technology suggests the use of a traveling wave resonant ring (TWRR) accelerator energized by two 1.2 MW continuous wave (CW) L-band klystrons (1249 MHz RF) to produce an electron beam with an energy of 10 MeV and a current of 100 mA. The average beam power is 200 kW-1 MW for the duty factor 20-100%. At full beam loading the accelerator is 65% efficient and is operable at room temperature.

The TWRR was selected to enhance the threshold current of beam break-up and to get high accelerator efficiency that results from the low value of attenuation constant and high field multiplication factor which are permitted only with TWRR. The advantages of using TWRR rather than a standing wave accelerator guide are simplicity of cavity structure, larger aperture size, ease of fabrication, and easy mechanical separation from the recirculating wave guide. All these things make it easy to handle under a high radiation field.

The klystrons are driven by 90 KVDC power supply to produce 1.2 MW RF with the efficiency of more than 65%. The 1.2 MW RF power is fed into four TWRR through two 3dB directional couplers.

The injector consists of a 200 KVDC electron gun, two magnetic lenses, a RF chopper, a chopper slit, a prebuncher, and a buncher. A peak current of 400 mA with beam energy of 200 KeV is required for the electron gun from the chopper and the buncher system design.

The accelerator section consists of seven accelerator guides. Each unit of accelerator section forms a TWRR. Each of the accelerator guides, of which the length is 1.2 m, contains 13 $2\pi/3$ mode cavities and two coupling cavities. All accelerator guides are constant gradient structure types under the condition of 100 mA beam loading. A straight waveguide was used instead of a phase shifter.

The first klystron energizes a buncher and three accelerator guides while the second klystron energizes the remaining four accelerator guides. The RF power fed into the buncher and each accelerator guide are 220 to 250 KW, respectively. Table 5 summarizes the characteristics of the 1 MW beam linac used at PNC.

Beam Flux Requirements

Calculations show that efficient (γ,n) incineration of Cs^{137} and Sr^{90} requires a gamma flux of only $10^{18}\ \gamma/\text{cm}^2\text{sec}$ to accelerate the time of decay by 180 times.[11]

The number of nuclei (γ,n) reacting during the irradiation can be determined by the following differential equation:

$$dN_i/dt = -(\lambda_i + \sigma_i\phi)\,N_i + \sum_{j\neq i}^{N_a}(\lambda_{ji} + \sigma_{ji}\,\phi)\,N_j, \quad (6)$$

$$i = 1,2,\ldots, N_a$$

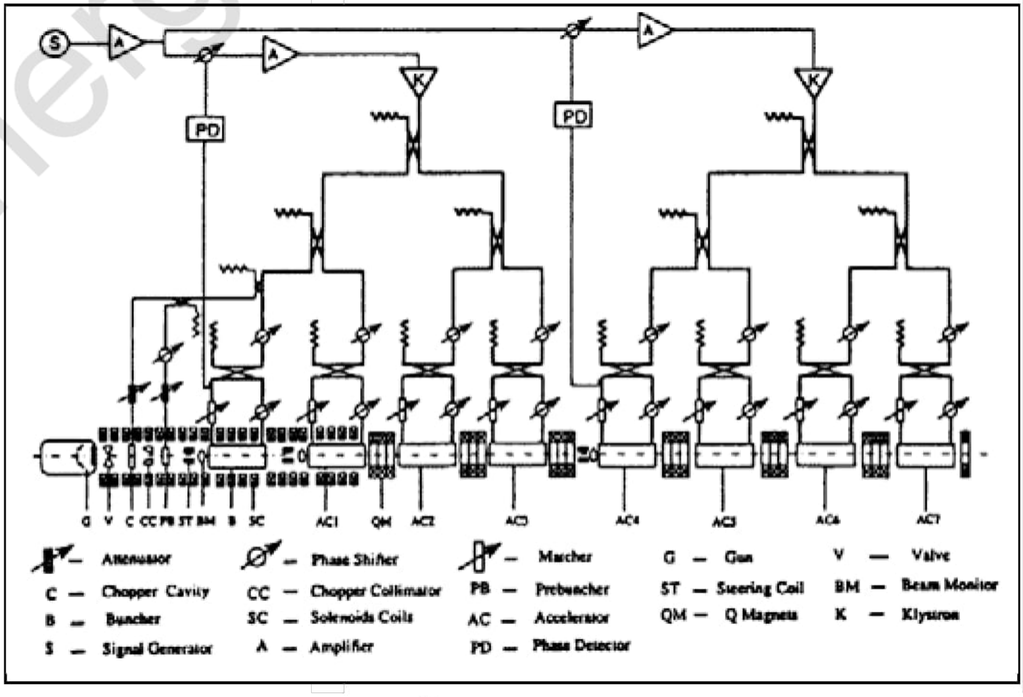

Figure 6. Schematic layout of CW electron linac.[10]

Table 5. Accelerator Specifications[10]

General	
Operation Mode	Continuous Wave
Energy	10 MeV
Beam Current	100 mAmp
Total Length	18 meters
Normal Emittance	50% mm mrad
Energy Spread	1%
Accelerator Section	
Type	Traveling Wave Constant-Gradient
Mode	2π/3
Frequency	1249.135 MHz
Gain (max)	1.4 MV/m to 2.0 MV/m
Number of Accelerator Guides	7
Resonant Ring	
Transmission (no load)	0.946
Transmission (load)	0.850
Multiplication (no load)	3.0
Multiplication (load)	2.0
Klystron	
Number of klystrons	2
Power	1.2 MW
Beam Voltage	90 KV
Micro-Perveance	0.8
Gain	50 dB
Efficiency	65%
Modulation	Modulating Anode

where

N_i = number of the ith nucleus,
λ_i = decay constant of the ith nucleus,
σ_i = total photonuclear cross section of the ith nucleus
λ_{ji} = decay constant from the jth nucleus transmuting to the ith one,
ϕ = γ-ray flux,
N_a = number of nuclei considered in the model.

Using the matrix representation, Equation (6) is written as follows:

$$d\mathbf{N}/dt = \mathbf{A} \bullet \mathbf{N}, \tag{7}$$

where

$$A_{ji} = \begin{cases} -(\lambda_i + \sigma_i\,\phi) & (i = j), \\ \lambda_{ji} + \sigma_{ji}\,\phi & (i \neq j). \end{cases}$$

The matrix of the nuclei **N** at the time $t = \Delta t$ can be obtained by the Taylor's expansion:

$$\mathbf{N}\,(t + \Delta t) = \mathbf{N}(t) + \sum_{r=1} (\Delta t)^r\,/\,r!\; d\mathbf{N}^{(n)}\,(t)/dt, \tag{8}$$

where $d\mathbf{N}^{(n)}(t)/dt$ is the rth derivative of $\mathbf{N}(t)$.

Combining Equations (7) and (8), we can obtain $\mathbf{N}(t+\Delta t)$ as follows:

$$\mathbf{N}(t + \Delta t) = \mathbf{N}(t) + \sum_{r=1}(\Delta t)^r/r!\;\mathbf{A}^r\mathbf{N}(t). \tag{9}$$

The matrix **A** contains two kinds of data: the decay constants and the photonuclear cross sections.

Figure 7 shows the products produced by photofission of U^{238} by 10 MeV x-rays.[10] The U^{238} itself may be used as both the gamma converter and the target. That is, eliminate a separate electron to gamma converter and use the target material itself as the x-ray source. The advantage here is the recovery of the heat normally dissipated in the converter, which is on the order of 70% of the beam energy.[12]

Conclusion

The (γ, *f*) and (γ,n) incineration of spent nuclear fuel provides an efficient and reasonable method for disposal of radioactive waste while providing a relatively cheap and safe source of power at the same time. No new technology needs to be developed since we currently have all the required technology available to us. A small proof of principle accelerator driven reactor could be built using known engineering with reasonable assurance and confidence that it will work as designed. Such a reactor may be fueled by current nuclear waste stockpiles, spent nuclear fuel, natural U^{238}, or natural Th^{232}. Matsumoto ran the calculations to show it is theoretically feasible. Kase ran the feasibility experiment that provided proof of feasibility on the laboratory scale. Safety is high, fuel is cheap and abundant, now all that remains is to apply the technology and build an experimental accelerator driven reactor.

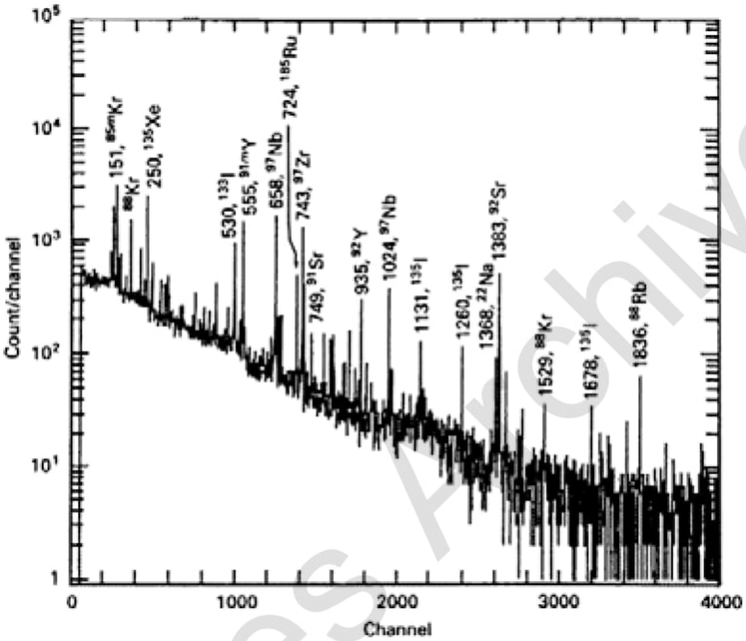

Figure 7. Products produced by photofission of U^{238} target with 10 MeV photons.

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About the Author

Dr. Brown is well-known for his contributions to isotopic generator research, especially related to direct energy conversion. He is a research scientist with more that fifteen years experience at public and private research facilities. He is the member of several professional societies including The American Nuclear Society and the American Institute of Aeronautics and Astronautics. Dr. Brown holds five U.S. patents and many others worldwide. His conference presentations include: Symposium on Space Nuclear Power and Propulsion-NASA; National Technology Transfer Conference-NASA; Intersociety Energy Conversion Engineering Conference; American Nuclear Society. He is the author of several periodical articles as well as six technical books.

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Deuterium-Based Radiationless Cold Fusion

Talbot A. Chubb* and Scott R. Chubb**

Deuterium-based radiationless cold fusion does not violate the known laws of science. It does not violate conservation of energy or momentum, and is not a perpetual motion machine. Many high energy physicists think that radiationless nuclear energy release is impossible, based on their experience in high energy particle and hot plasma systems. These latter are free particle systems. In free particle systems there is an effective separation of the nuclear and electromagnetic forces. The electromagnetic force includes the Coulomb repulsion between charged particles (*e.g.*, deuterons) that normally prevents nuclear reaction except at high energy (*e.g.*, in the Sun or other hot plasma). However, the separation between nuclear and electromagnetic forces does not fully apply to neutralized deuterons embedded inside a metal lattice. Such deuterons constitute a system of bound particles, which can share momentum with the embedding lattice. This sharing of momentum occurs in all Mössbauer-effect nuclear transitions.

The quantum mechanics of bound particles leads to a coupling between the nuclear configuration, the charge density distribution of the particles, and the lattice. We are concerned with a nuclear reaction where bound deuterons are the fuel and bound helium atom nuclei are the reaction products. In its normal chemical state, a deuteron occupies a small interstitial volume within the metal lattice. This charge distribution interacts with the lattice. When a nuclear reaction occurs, the reaction changes the volume occupied by two deuterons into that occupied by a helium nucleus. There is then a change in the distribution of positive charge density within the solid. The result is that in bound systems the nuclear and electromagnetic interactions are coupled, *i.e.*, not fully separable. This coupling has only a small effect when one considers side-by-side deuterons resembling an embedded D_2 molecule.[1] In this case there is no deuteron-deuteron wave function overlap and no nuclear reaction. But the coupling becomes important in band state systems containing superposed, wavelike deuterons, as described below.

In a bound system, a particle embedded in a solid has important distributed parameter characteristics. As discussed above, its charge is distributed over a volume. The spatial spread is described by a wave function density. Over this volume the particle behaves coherently, in the sense that when there is any change in the particle anywhere, the change occurs instantly everywhere.[2] In periodic systems, particles can take on the periodicity of the lattice and resemble a standing wave of charge. The particles can then be said to be spread throughout a crystal. When this happens, the particle is called a band state particle. This always happens with the conduction electrons in a metal. Some surface distributions of hydrogen on a metal are known to have band state characteristics. Unlike the situation in free space, within coherent volumes momentum is not necessarily conserved at a point. But momentum is always conserved globally, as occurs in the calculations of electronic bands using the Koringa-Kohn-Rostoker method.

Nuclear reactions generally involve particle pairs. Arguments against nuclear reactions, based on conservation of energy and momentum at a point or over a nucleus size volume, do not apply to pairs of band state particles. Band state atom systems are similar to the Bose-Einstein condensate system studied by Jaksch *et al.*[3] If one builds on the Jaksch *et al.* analysis, it is seen that for a pair of superposed band state particles, the Coulomb repulsion potential energy summed over the coherence volume is inversely proportional to the coherence volume (crystal size). This is because the Coulomb potential is a long-range potential. This means that when imposed crystalline periodicity prevails, the Coulomb potential energy associated with particle-particle repulsion decreases, decreasing the effective repulsion force. In contrast, the attracting nuclear potential is a contact or short-range potential. When the associated nuclear potential energy is summed over the pair's coherent volume, the attracting potential energy is unchanged. When the particles are deuterons and the coherent crystal volume is sufficiently large (~0.01 micron), the Coulomb force is no longer able to prevent the attracting nuclear force from pulling the two band state deuterons together so as to form a band state helium nucleus. Nuclear reaction occurs. The reaction energy is dissipated in the scattering of large numbers of lattice electrons. The failure of nuclear physicists to properly consider these solid state quantum physics effects has led to the belief that nuclear reactions in the lattice cannot take place. This is an incorrect view. Moreover, since periodic order dominates band state particle reactions and the reactions couple their released nuclear energy to the conduction electrons of the metal, there are no high energy fragments, no radiation, and no neutrons.

References and Notes

1. It has been shown by Leggett and Baym [Leggett, A. J. and Baym, G. 1989. *Phys. Rev. Lett.*, **63**, 191] that side-by-side D-atoms in a lattice are less tightly configured than the side-by-side atoms in a D_2 molecule. Since the D-atoms in a D_2 molecule are kept apart by the Coulomb repulsion between the D-atoms, it is clear that the same situation will exist for side-by-side D-atoms in a lattice. Moreover, if the D-atoms were to undergo a nuclear reaction, the result would not be radiationless release of nuclear energy. A compound nucleus would form, as in plasma fusion. The compound nucleus would then fragment, releasing energetic neutrons or protons, as occurs in free space fusion. This behavior is clearly illustrated in muon-catalyzed fusion, where a very small "D_2^+ molecule" is produced, which undergoes fusion with release of the normal products of hot fusion.

2. Though bound particles are spread out over macroscopic volumes and contribute charge density over this volume, the charge and mass distribution continues to be quantized. When several spread out particles occupy the same volume of space, there are always only a whole number of particles present. When two or more particles of the same type occupy the same space with the same spatial distributions, they combine, forming an entity described by a many-body wave function. This entity can then divide itself into quantized sub-entities called quasi-particles, which are themselves quantized, just like the particles making up the merged group. In each quasi-particle there are portions of each of the original merged particle set. These portions act in concert (*i.e.* have a fixed phase relationship with each other). This behavior is called coherent, and the volume occupied by these quasi-particles is referred to as a coherence volume. The behavior of a single quasi-particle is the same as that of a single, spread out particle.

3. Jaksch, D., Bruder, C., Cirac, J. I., Gardiner, C. W. and Zoller, P. 1998. "Cold Bosonic Atoms in Optical Lattices," *Phys. Rev. Lett.*, **81**, 3108. Jaksch *et al*. analyze a many-body Bose atom system in an optical lattice. They use a two-particle Coulomb repulsion energy term equivalent to a sum over pairs with repulsion energy $U_{ij}=2\pi a_s\hbar^2\int d^3x|\psi i(x)|^2|\psi j(x)|^2/m$, where a_s is a constant that measures the strength of the repulsion of the two atoms if confined to a single unit cell, $\hbar$ is Planck's constant divided by 2π, the $|\psi i(x)|$ are single particle wave function amplitudes, m is the atom mass, and the integration extends over the coherent volume. For the idealized case of two deuterons in a metal crystal having a coherence volume consisting of N_{cell} identical unit cells, the $|\psi i(x)|^2$ for each deuteron is normalized to one over the full crystal coherent volume. It follows that each $|\psi i(x)|^2$ is normalized to $1/N_{cell}$ over a single unit cell. Integrating over a single unit cell, the contribution to U_{12} from each unit cell scales as $1/N_{cell}^2$. After summing over the full coherent volume, U_{12} scales as $1/N_{cell}$.

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Scott Chubb received his B.A. degree in physics from Princeton University in 1975 and his M.A. and Ph.D. degrees, also in physics, from the State University of New York (SUNY) at Stony Brook, in 1978 and 1982 respectively. He has been an author of more than fifty refereed publications in scientific journals, in areas related to solid state physics, general and special relativity, electromagnetic scattering, non-linear wave dynamics, statistical physics and nuclear physics. He also is an author of one patent, which was awarded in 1996.

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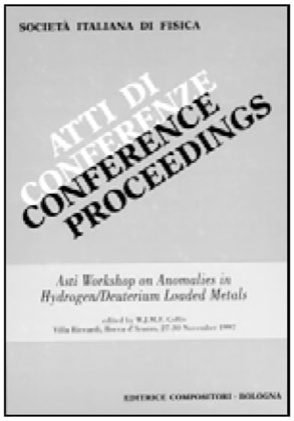
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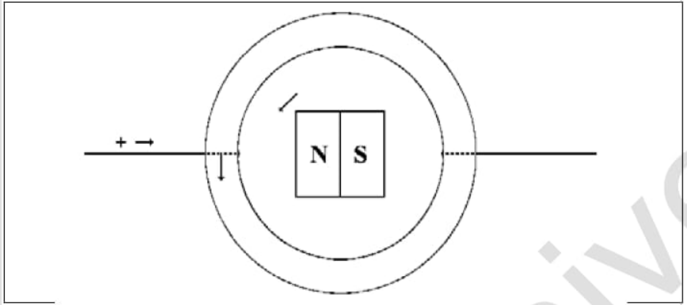

Figure 2. Brushes to inside of ring. Ring rotates counterclockwise. Magnet pair rotates counterclockwise.

magnet pair were dipped to a depth of about half its height.

Figure 1 shows what happens when the brushes are outside the ring and both ring and magnets are free to turn. The arrows show that the ring begins to rotate clockwise, and the magnets begin to rotate counterclockwise. Figure 2 shows the spooky configuration with the brushes to the inside of the ring. As depicted, the ring rotates counterclockwise and the magnets rotate counterclockwise, both starting from rest. (I have this all on videotape, by the way.)

So what gives? What are the magnets and the ring "pushing against" if not each other?

At this point most people look to the leads. It is suggested that, perhaps, either the magnets or the ring actually "push" on the leads, and then one pushes on the other. Most certainly, the ring does not push on the leads since the ring will move even if the leads are soldered to it (a fine, lightly suspended chain will work to show this). It also doesn't matter if the leads are brought in from the bottom or from the top instead of from the sides. This leaves the magnets to push off from the leads in some way.

There is also a problem with this idea. If the magnets are pushing on the leads then, via Newton's Third Law, the leads must also be pushing on the magnets. That pushing is what we're trying to show is still ultimately at work here, even though we've managed to make both the ring and the magnets rotate in the same direction at the same time. As anyone knows who has ever tried to push a stuck car, it's a lot easier to get the car moving if you have something to brace your feet against, like hard soil or a convenient rock, than if you're standing on ice.

I tested this "pushing on the leads" hypothesis. With the ring fixed and the magnets free to turn, I replaced the stiff tungsten rods with some flimsy strips of aluminum foil, no more than a quarter inch wide and maybe six inches long. Bear in mind that the magnet pair weighs in at a bit over half a pound. I expected that, with flimsy leads, once the circuit was closed and the magnets and the leads were pushing on each other, the foil strips would flick away, imparting very little push to the magnets. This is just like having your feet slip out from under you while standing on ice and trying to push a car.

This is not what happened. With a short burst of current (as described above) what did happen is that the magnets took off spinning like they normally do. This is because the magnets are interacting with the ring. Once the magnets are spinning, you can even take the foil leads and try to stop the rotation with mechanical contact of the leads against the magnets. It takes a while to get the magnets to stop turning this way, though—these are just flimsy pieces of foil, after all. So, again, *what gives*?

That analysis is for another report.

❑❑❑

Nuclear Processes in Palladium Deuteride

H.C. Josephs*

Since the announcement by Fleischmann and Pons[1] in 1989 of the discovery of the anomalous production of neutrons, tritium, and excess heat in a room-temperature electrolytic cell, most experimenters and theorists have followed their lead, assuming that the nuclear reactions necessary to produce these results were a new form of deuterium-deuterium (dd) fusion. The role of the metal host appears to have been regarded as an environment for bringing the deuterium atoms into sufficiently close proximity so that fusion can occur. In this paper, the role of the host atoms will be considered and reactions involving palladium and its daughters will be proposed. It is proposed that the fusion reactions can be catalyzed, in a manner similar to the solar carbon cycle, by the addition of a small amount of rhodium to the palladium.

Introduction

Except for atomic mass and energy, the units used in this paper are the MKS or International System of Units. Atomic mass and energy are expressed in mega-electron volts (MeV). Atomic mass may also be expressed in atomic mass units (amu). To use these units in general calculations they must be converted to MKS units using the conversion factors:[2]

1 amu = 1.6605402 x 10^{-27} kg = 931.49432 MeV
1 MeV = 1.6021892 x 10^{-13} joules
Electron mass = 0.51 MeV
Proton mass = 938.27231 MeV
Neutron mass = 939.56563 MeV
H-2 mass = 1876.12439 MeV
H-3 mass = 2809.433 MeV
He-3 mass = 2809.415 MeV
He-4 mass = 3728.399 MeV

It is an established fact that nuclear reactions can occur at room temperature. Neutron capture is a form of room-temperature nuclear reaction, which has been known since the discovery of the neutron in 1932. Because there is no Coulomb barrier to the passage of neutrons, very low energy neutrons are capable of entering the nuclei of even the most massive atoms. The neutron-induced fission of uranium-235 is an everyday example. In fact, for most nuclei, the probability of a neutron being captured, known as the capture cross section, increases as the velocity of the neutron decreases.[3] Thus, neutron capture is a well-studied, near room-temperature nuclear process.

The absorption of energetic deuterons by various atomic nuclei has also been well-studied.[3] Because the deuteron is positively charged, it is strongly repelled by the positive charge of another atomic nucleus when they approach one another. The energy required to overcome this repulsive force is known as the Coulomb barrier. For dd encounters this barrier energy is about 480,000 electron volts (eV)[3] compared with 0.026 eV for the energy of room temperature particles. It is not necessary for deuterons to have hundreds of kilo-electron volts of energy in order for there to be a non-zero probability for fusion to occur, but only that they be close enough together for the "tail" of one deuteron's wave function to extend into the region where the nuclear strong force of the other nucleus dominates. Both of these processes will be discussed below.

Neutron Capture Versus Deuterium Fusion

The capture of a neutron increases the atomic mass of the nucleus by one mass number unit without changing the atomic number. This is usually accompanied by the release of six to nine MeV of energy for nuclei more massive than neon.[3] If this excess energy, known as the binding energy per nucleon, does not cause immediate ejection of the neutron, then the nucleus is left in an excited state from which it usually recovers by emitting a broad spectrum of gamma radiation. This occurs above the peak of the binding energy curve, near iron-56, as well as below it.

In some cases the daughter nucleus is unstable and decays by processes known as β-decay[3] or orbital electron capture. In β-decay, a neutron is converted to a proton by the emission of an electron, and an antineutrino or a proton is converted to a neutron by the emission of a positron and a neutrino. In orbital electron capture, a proton is converted to a neutron by the absorption of an electron from one of the innermost orbits of the atom. A neutrino is also generated in this process. All three processes release additional energy, some of which is carried off by a β-particle (which term includes both the electron and the positron) and a neutrino (which term includes the antineutrino). The rest is carried off by x-ray and gamma radiation. The half-lives of these decay processes typically range from seconds to days. These processes change the atomic number by one unit without changing the mass number.

The net combined effect of these processes is that the mass number increases by one and the atomic number of the nucleus either increases or decreases by one. For example, by this process of neutron capture followed by β-decay, palladium-102 (Atomic No. 46) could be transmuted to rhodium-103 (Atomic No. 45) or palladium-108 could be transmuted to silver-109 (Atomic No. 47).

The primary question here is whether or not, and if so *how*, a neutron can be freed from a deuteron at room temperature. The binding energy of the deuteron is 2.22 MeV, so at least that much energy is needed. In high energy, or "hot" interactions, dd fusion occurs after one of the deuterons has been split. The other then captures either the proton or the neutron, forming helium-3 or hydrogen-3. The probabilities of these two events happening are about equal because deuterium has a very low capture cross section for hot neutrons as well as for protons. Proton capture therefore frees a neutron, just as neutron capture frees a proton, but these reactions can only happen at high energy. The 0.025 eV available at room temperature is totally insufficient to free either. The dd capture cross section is extremely small at thermal energies.[4]

In a metal crystal, however, the situation is different. The extra electrons which the deuterium contributes to the palladium crystal lattice are located in the conduction band and are free to move throughout the crystal. The deuterons are also free to wander through the crystal, but are not as mobile as the electrons. They are, however, much more mobile than atoms, which are about 100,000 times as large. Because of their opposite charges, the deuteron will tend to attract a cloud of negative charge around it, canceling out its positive charge for distances greater than something on the order of the classical radius of the electron orbit, 5.3 x 10^{-11}m.[2] The deuteron and its associated electron cloud is therefore electrically neutral and free to drift through the Pd, provided that it avoids the fixed locations of the Pd nuclei. The average distance between palladium nuclei, r, can be estimated from the atomic weight, M, and the density, δ: $r = (M/\delta N)^{1/3}$, where N is Avogadro's number—$r \approx 2.5 \times 10^{-10}$m. The distance of closest approach of one deuteron to another is roughly twice the orbital shielding radius, or 10^{-10}m, because the Coulomb forces increase extremely rapidly once the shielding begins to break down. Note that this is also about equal to the lattice spacing.

In order for one nucleus to fuse with another, in addition to all the conservation laws having to be satisfied, the nuclei must approach each other closely enough that their nuclear wave functions overlap. Then they must remain there long enough, or have enough encounters, that there is a statistical probability that one will appear within the capture cross section of the other. If there is a cross section listed for a particular nucleus and particle in the *Handbook*,[2] then that capture has been observed. This means that all the conservation laws are guaranteed satisfied except for energy and momentum. For a thermal deuteron with velocity[3] given by $v = (3kT/m)^{1/2} = 1900$ m/s, the deBroglie wavelength[3] is $h/mv = 1.03 \times 10^{-10}$m, about equal to the approximate dd distance of closest approach. Here k is Boltzmann's constant, T is the absolute temperature, m is the mass of the deuteron, and h is Planck's constant. At this distance the barrier potential[3] is only 13.6 eV, but that is still 520 times the thermal energy of the deuteron, Figure 1a. The deuterons cannot approach any closer, and, because both the thermal deuteron and neutron capture cross sections are so small, they would have to remain there for a very long time for the capture to occur. One can therefore conclude that room temperature dd fusions or neutron stripping cannot occur through dd interactions with a significant probability.

The situation concerning the palladium-deuterium interaction is much more favorable. The classical radius of the innermost electron orbitals is 1.15×10^{-12}m for Pd as compared to 5.3×10^{-11}m for deuterium (D), smaller by the atomic number, Z = 46. The binding energy of the innermost electrons is 28.8 keV, larger by Z^2, so they are much better able to provide shielding for the thermal deuterons. All of the 46 Pd electrons are between the D and Pd nuclei, so the D is 100% shielded at the maximum distance from the Pd. The distance between the two nuclei at maximum separation is only half the lattice spacing, about 5.4×10^{-11}m, so the deBroglie wave of the D overlaps the Pd nucleus a lot. The D is able to approach the Pd nucleus much more closely, because of the more effective electron shielding, so the wave function overlap can increase considerably, Figure 1b. Due to the voltage drop across the surface barrier at the cathode, the deuterons at the surface may have considerably larger than thermal energy. This may increase the probability of deuteron capture.

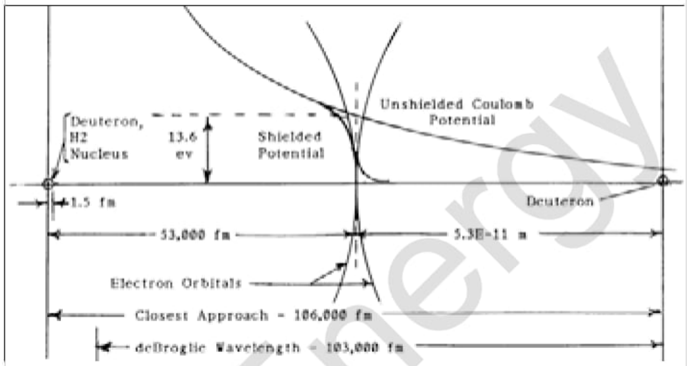

Figure 1a. Distances are expressed in femtometers, fm (also called Fermis), to emphasize the dramatic differences between the radius of the deuteron and the distance of closest approach. The distances are not to scale. The shape of the potential curves and distances are only intended to be illustrative.

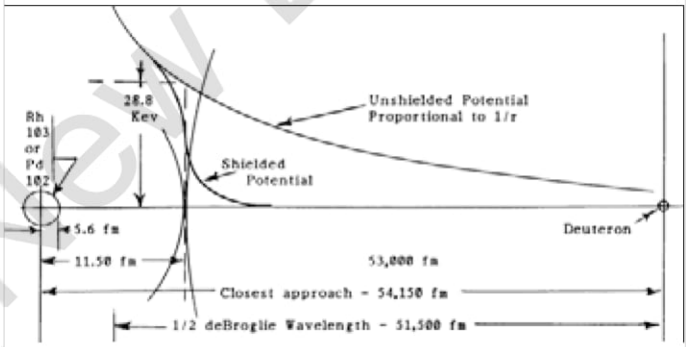
28.8 Kev
Rh 103 or Pd 102
Unshielded Potential Proportional to 1/r
Shielded Potential
5.6 fm
Deuteron
11.50 fm
53,000 fm
Closest approach - 54,150 fm
1/2 deBroglie Wavelength - 51,500 fm

Figure 1b. Illustrative comparisons of deuteron-deuteron vs deuteron-palladium barrier potentials, inter-nuclear distances, and the effect of shielding. Note that the probability of capture or splitting of a deuteron by either palladium or rhodium is much greater than the probability of deuteron-deuteron fusion.

The thermal neutron capture cross section for the six stable isotopes of Pd[2] varies from zero barns for Pd-104 to twenty-eight barns for Pd-106. (One barn (b) = 10^{-28}m^2.) The binding energy per nucleon of Pd is 8.3 MeV, so there is adequate energy available to split the deuteron. Thus, there is a non-zero probability that the Pd nucleus will capture a neutron from a D atom with the release of about 6 MeV, the Pd binding energy per nucleon minus the D binding energy. When the deuteron is split, the ejected proton carries off most of the energy, which is then transferred, within a few microns, to the Pd lattice through interaction with electrons. The daughter nucleus, formed when the Pd captures a neutron, is usually left in an excited state.

Neutron-Induced Nuclear Processes in Palladium

It must be said that the neutron capture process described above has a rather low, although non-zero, probability of occurring. The deBroglie wavelength is just barely enough to overlap the Pd nucleus by one-half wavelength. Three of the six Pd isotopes have capture cross sections of less than 0.3 b. These three isotopes—104, 108, and 110—account for half of all naturally occurring Pd, so half of all Pd is essentially inactive regarding nuclear processes involving neutron capture. Even for the remaining three isotopes, neutron capture will be a rare event because of the small wave function overlap. But it is worthwhile examining what these rare events lead to.

Let us begin with the heaviest, Pd-106, which has the highest probability to capture an occasional neutron, because its cross section is 28b. The excited Pd-107 nucleus which results then β-decays to silver (Ag-107), which is stable. The process emits beta rays, gamma rays, and neutrinos, which between them carry off most of the energy. Some of the energy gets deposited in the metal by electron absorption and phonon generation. These, however, will be generated at a very slow rate because Pd-107 has a half-life of 6.5 million years. No capture cross section is listed for Pd-107, so this appears to be a nuclear dead end.

Pd-105 has about as good a chance as Pd-106 of capturing a neutron; its cross section is 22b. When this happens, it is converted to an excited form of Pd-106. After the excitation energy is dissipated, the new Pd-106 nucleus is identical to all the others. Although some radiation is produced, it is another dead end.

Pd-102 has the lowest cross section of the three "active" isotopes, 3b, and also has the lowest natural abundance, comprising only 1% of all the Pd atoms. It therefore has the lowest probability of capturing a neutron. But after being bathed in a sea of thermal deuterons for days to months, a few of these rare and reluctant isotopes may have accomplished the task of capturing a neutron and becoming Pd-103. The numbers of reactions are so small that the energy yield is unimportant, as it also is for Pd-105 and Pd-106. What is important is that Pd-103 is transmuted by orbital electron capture, to rhodium (Rh-103), the only stable isotope of Rh. The half-life of this decay is seventeen days. For any individual Pd-103 nucleus, the actual decay time is anywhere between zero and infinity. About half would decay in the first seventeen days.

Rh-103 has a thermal neutron capture cross section of 140b, or almost fifty times that of Pd-102. Therefore, these few, newly-created Rh-103 nuclei have a much better chance of capturing a neutron than their parents. When they do, they become Rh-104, which in its excited state has a neutron capture cross section of 800b but a half-life of only about 4.4 minutes. Undoubtedly some of these β-decay to Pd-104 before anything else can happen but, because of the large cross section, some will probably capture another neutron. When this happens they become an excited state of Rh-105, which has a half-life of 45 seconds. These decay to the ground state, which has a capture cross section of 5000b(!) and a half-life of 35.4 hours. Rh-105, therefore, has the ability to capture free neutrons very quickly, and to continue to do so for a relatively long time. The β-decays of Rh-104 and -105 lead to Pd-104 and -105, which are both dead ends. None of these reaction paths seem to produce the sudden bursts of significant amounts of energy that experimenters have been reporting. But wait. . .

Experimental Results

The author has been doing theoretical work in the "cold fusion" area since April 4, 1989; experimental work began on June 14, 1989. Both continued until February 1996, when a personal relocation required a temporary suspension of experiments.

One of the more "successful" experiments was begun on July 15, 1991. The reaction chamber was of the Fleischmann-Pons type with a 2 cm^2 pure Pd cathode with ten inches of Pt wire wound around it as the anode. The electrolyte was 100 ml of pure D_2O. The reaction bottle and insulating electrode supports were made of Pyrex. The current was a half-wave rectified sine wave with an average value of 31 mA. The voltage was 48V average and 150V peak. The current, voltage, and waveform were all monitored. The potential difference between the electrolyte and the cathode was not measured. Temperature at this time was being monitored with an alcohol-bulb thermometer. A rubber stopper was used to seal the reaction chamber. The Pyrex-insulated electrical wires, a vent tube to a condensation bottle, and the thermometer were passed through holes in the stopper, which were then sealed. The radiation was monitored with a Geiger-type monitor, sensitive to alpha, beta, gamma, and proton radiation. It is assumed that the majority of ionizing radiation that penetrates to the Geiger tube is gamma rays. The number of particles or photons was accumulated continually with an electronic pulse counter. The accumulation time was checked with a stop watch. The background count was established at about 850 counts per hour.

To quote from the laboratory notebook:

> At 8 a.m. on August 20, I entered the lab after having left things unattended for almost eighteen hours. It was immediately obvious that the counter was counting at an abnormally high rate and was displaying an unusually high count. The D_2O in the chamber was frothy and churning gently. The inside of the glass above the water line was clouded with condensation. "Rays" of bubbles (very tiny) were frequently streaking or radiating out from the Pd. I picked up the reaction flask and gave it a gentle swirl to clear away the condensation to be able to read the thermometer. The flask was cool (normal) to the touch. When I did this everything stopped. The counting rate dropped to normal. The froth floated to the surface and disappeared.

The thermometer bulb had broken, so it was impossible to determine the electrolyte temperature. The number of counts recorded in seventeen hours and forty-five minutes was 157,765 for a counting rate, averaged over the entire eighteen hours, of 8888 counts per hour, or about ten times background. If the activity had begun only a couple of hours before being observed, the actual counting rate could have been as much as ten times the figure reported. If the rate had been much higher than that, words like "the counter was going wild" would have been used to report the observation. Because the window of the Geiger tube was 1 cm^2 and it was located about 3 cm from the Pd, the actual event rate, assuming each count represented one event of some type and that the gammas, betas, or protons (whichever were being counted) were emitted in an omnidirectional manner, was about 1,000,000 per hour. Even if these were 24 MeV events, the power output was only 1 nanowatt. If the radiation were generated by only a fraction of the actual events, the power generated would have been considerably higher.

The results that various other experimenters have reported[5] have included, in addition to sporadic and unreproducible surges of heat:

1. A period of one to six weeks from the beginning of electrolysis to the first episode.
2. A wide variety of sporadic and unreproducible effects which, if real, could only be due to nuclear reactions, *i.e.* neutrons, gamma rays, helium, tritium, rhodium, silver, and other contaminants in the Pd, and cracks and halos in the Pd.
3. The amount of radiation and neutrons produced are many orders of magnitude too small to account for the heat.
4. Extreme sensitivity to physical disturbances.
5. Continued production of heat after the cessation of electrolysis.
6. Amounts of helium and tritium much too small to account for the heat.[7]

A Proposed Explanation of Experimental Results

To explain these widely-reported results one needs to consider the concept of nuclear halo states. It has been shown that the protons and neutrons, which make up the nucleus of an atom, have a shell structure similar to the shell structure of the electrons in an atom. The density of matter in the nucleus decreases gradually to zero over a distance known as the skin thickness.[3] For most atoms more massive than carbon, the skin thickness is ≈2.3 fm. It has also been shown that it is possible, at least in light nuclei, for captured neutrons to occupy energy states, known as halo states, far outside the nuclear potential well.[6]

The possible existence of halo states in Pd or Rh has not been investigated, but the fact that there are nine Rh isotopes listed, which are more massive than Rh-103, shows that there are at least that many permitted neutron states outside the highest occupied shell in Rh-103. Presumably they are all within the nuclear potential well. The ability of Rh-105 to capture neutrons from as far away as 400 fm (corresponding to the cross section of 5000b) suggests that there may well be halo states outside the calculated nuclear radius[6] of 5.6 fm.

Whether or not the above is true for both Pd and Rh, Rh-103 with a cross section[2] of 140b has about forty-six times the probability of neutron capture as Pd-102. There is enough binding energy per nucleon in the Rh to split the deuterium, eject the proton, and leave the Rh-104 in its excited state which has a half-life of 4.4 seconds and cross section of 800b. With this high cross section, Rh-104 has an even higher probability of splitting a deuteron, capturing the neutron, and ejecting the proton, which carries off some of the excess energy.

The half-life of the excited state of Rh-105 is forty seconds. If the Rh-105 should then capture a deuteron while the neutron is still in this excited state, the two particles will then be in extremely close proximity and will undoubtedly and very quickly[3] fuse to form a tritium nucleus. The newly-formed triton will then be promptly ejected. The total yield is 4.03 MeV, but how it is divided between the proton and the triton is not clear, since they are not ejected simultaneously. The division will depend on the energy of the excited state of Rh-105.

The cross section of the excited state of Rh-105 for either neutron or deuteron capture is not listed, but the ground state, which has a half-life for β-decay of 35.4 hours, has a neutron capture cross section of 5000b. If the neutron cross section of the excited state is similar, then the deuteron cross section would also be expected to be high, as this is no longer a low-energy process. In high-energy processes neutrons and protons behave in much the same manner. If the process proceeds as described, the Rh is returned to mass number 104 in the excited state, where it is available to start the process over again. Here the rhodium acts as a catalyst to facilitate the generation of tritium, but is not itself consumed until it finally decays to Pd-104 or Pd-105 and the process stops. The energy released is the same as the 4 MeV per fusion reaction achieved in dd production of tritium. Most of the energy is carried off by the ejected proton and triton (the proton comes out first). This energy is transferred to the lattice through interactions with electrons within a few microns of the location of the Rh atom. The remainder, about 2.8%, is transferred to the Rh nucleus in a reaction to the ejection of the fusion products. The energy is shared[3] in inverse proportion to the masses of the reacting particles, $W_1/W_2 = m_2/m_1 = 3/105 = .028$.

A very similar process could take place from the ground state of Rh-105, where the half-life and neutron cross section are known. The cross section for deuteron capture in the ground state is not listed. The process would take place after the 45 second decay time of the excited state.

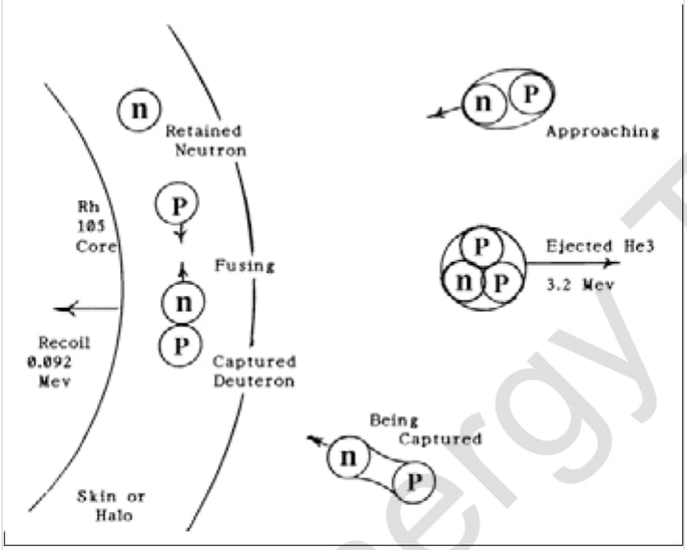

Figure 2a. Two captured deuterons fuse and a He-3 nucleus is ejected. The excess neutron is retained.

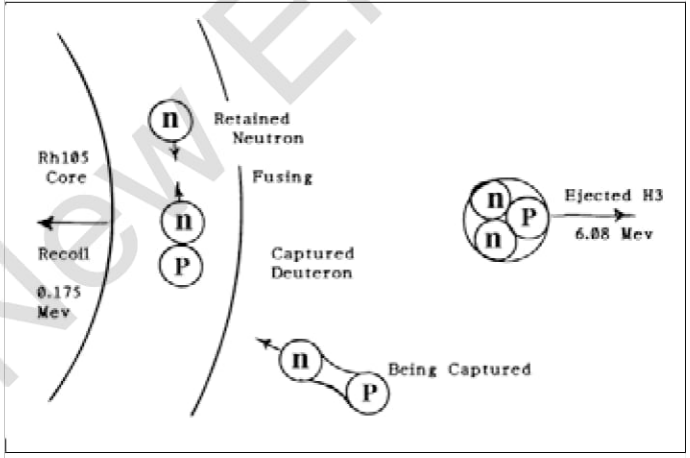

Figure 2b. A third deuteron is captured, fuses with the retained neutron, and is ejected as a triton. Total energy is 9.525 MeV.

Helium-3 could be produced by a similar process, because of the similarity of protons and neutrons in the nucleus. In this case, a deuteron would be captured first and a second deuteron later. The excess neutron could either remain in the excited Rh-105, be ejected in advance of the He-3, or be ejected simultaneously with it.

An alternative process would be the capture of two deuterons by Rh-105 in its ground state. As the barrier potential of Rh is 100 MeV, the deuterons are in very high energy conditions with the nuclear forces of the Rh dominating the dd Coulombic repulsion. In this process He-3 and H-3 would be produced in equal amounts. One of the two captured deuterons is split, the proton fuses with the other, and the resulting He-3 nucleus is ejected. The neutron is retained in an excited state, Figure 2a. The energy of the excited neutron may vary depending on which state it occupies.

A third deuteron is then captured, fuses with the retained neutron and ejects the resulting triton, Figure 2b. The total released energy is the mass of three deuterons minus the sum of the masses of the triton and He-3 nucleus, 9.525 MeV, with the Rh nucleus absorbing 2.8%. The H-3 and He-3 would therefore share the remaining 9.26 MeV in some way which would depend on energy of the retained neutron. In this process there are no ejected protons or neutrons, no gammas, x-rays or neutrinos, only H-3 and He-3. The results look a little strange as one is accustomed to seeing the results of high energy collisions, where four D's produce one each p, n, H-3, and He-3 plus 7.3 MeV. Here we have a more efficient process in which three D's produce only He-3, H-3, and 9.52 MeV. The difference is that we have gained the 2.22 MeV binding energy of the extra deuteron. The Rh is not consumed, but is immediately ready to catalyze another fusion reaction.

Another possible reaction is the complete fusion of two deuterons to form He-4, with the release of 24 MeV of energy. When the dd reaction is performed in vacuum, the He-4 production requires the release of high energy gamma rays to maintain the energy and momentum balance, Figure 3a. When the He-4 is formed in close proximity to the Rh, actually within the nuclear potential well, the Rh nucleus will perform the energy-momentum balance, Figure 3b. Some of the He-4 nuclei (α-particles) would be ejected with 96% of the reaction energy, 22.9 MeV. This energy is insufficient to disrupt the He-4 nucleus, but would be very quickly absorbed by the Pd electrons. The He-4's, which are not immediately ejected, decompose back to two H-2's. (This is what actually happens to the majority of He-4 produced in high energy dd impacts, because the 24 MeV is enough to disrupt the He nucleus in about 10^{-24} sec.) The two d's could then engage in the H-3 or He-3 reactions. In the process under consideration, more of the He-4 may survive because of the different environment in which it is generated. What is being created is a compound nucleus consisting of Rh-105 and highly excited He-4, in the environment of the nuclear potential well of a massive atom.[8]

The Rh-105 would be returned to its own ground state by any of the three reactions above. Because the Rh does not have to decay from an excited state, no gamma or x-rays are produced and no time delay is involved. The Rh is ready to immediately catalyze another dd fusion.

Summary

Of the reactions proposed, it would appear that the most capable of explaining the reported experimental results are:

1. The absorption of a neutron and ejection of a proton by Rh-104, followed by the absorption of a deuteron and ejection of a triton by Rh-105, Figure 4a. This would result in the production of heat, H-1 and H-3 via the long loop. The Rh is not consumed until it decays to Pd. No free neutrons are produced. Since the reactions occur in the excited states of both the isotopes involved, no gamma or x-rays are produced and no decay-time delays are involved. When β-decay does occur, the process stops.

2. The absorption of two deuterons by Rh-105 followed by the immediate ejection of a triton, an He-3 nucleus, or an alpha particle, Figure 4b. This results in the production of heat, H-3, He-3 and He-4 via the short loop with no detectable radiation, including no neutrons, other than that produced by tritium. If He-3 is produced, the excess neutron is retained in the Rh, where it will participate in the production of tritium with the absorption of the next deuteron.

In order to produce one watt of thermal power, the reaction rate would have to be about 10^{12} reactions per second. As the time required for the fusions to take place once the necessary captures have taken place is of the order of 10^{-15} sec, the rate of heat production is limited by the availability of deuterons. Because the deuterons are essentially diffusing toward a point (a Rh nucleus), the reaction rate will be diffusion-limited. Except for this limit, it appears that only one Rh-105 atom per experiment would explain most of the experimental results. For the diffusion-limited case, based on the author's one "successful" experiment, about a billion Rh-105 nuclei per watt would be needed.

Because the production of heat is concentrated in a volume a few microns in diameter (the range of protons, tritons, He-3, or α particles in Pd), the out-diffusion of heat (phonons) away from the source will tend to deter the in-diffusion of deuterons. Since the process could continue for days due to the long half-life of Rh-105, heat may build up until lattice damage occurs, further limiting the in-diffusion of deuterons. The process therefore tends to be self-limiting. In any case, the process will come to an abrupt halt when the Rh decays to Pd or if the Rh loses physical contact with the host metal.

What actually happens is probably some combination of all of the processes mentioned here, plus several not mentioned.

The experimental results listed previously are explained by rhodium-catalyzed fusion as follows:

1. The delay of one to six weeks before the first evidence of nuclear activity is due to the low probability of a neutron being captured by Pd-102 and the randomness of the timing of the β-decay that leads to Rh-103.

2. The wide variety of results is due to the wide variety of nuclear processes that can actually happen with low probability. The sporadic and unreproducible nature of the results is due to the randomness of low-level nuclear events where there is none of the averaging effect of large numbers. The production of a large variety of nuclear by-products is explained by well-known physics, once the possibility of deuteron splitting and neutron absorption in a Pd lattice is admitted. The cracks are due to the lattice swelling that occurs when hydrogen is absorbed and released by the palladium. The halos are the lattice damage resulting from heat being concentrated in a small volume.

3. Neutrons are produced in conjunction with He-3, but are usually retained in the Rh nucleus. Gamma rays, β-rays, and x-rays are produced only by the infrequent decay processes and not by the rhodium-catalyzed fusion process described above. The protons, tritons, He-3, and α-particles, which are produced by the rhodium-catalyzed fusion processes, have too short a range in metals to be detectable outside the reaction chamber unless the Rh is right on the surface, as it appeared to be in the case of the author's reported experiment.

4. The most likely place for the neutron absorption to occur in a Fleischmann-Pons type reaction chamber is at or near the surface of the Pd where the concentration of deuterons is the highest. Because the physical location where there was a Pd-102 atom is where a Rh-105 atom will eventually reside, and that is also where there will be the most thermal damage to the Pd crystal, the site of the most activity will be mechanically very delicate and easily removed from the surface.

5. Because of the thirty-five hour half-life of Rh-105, the cat-

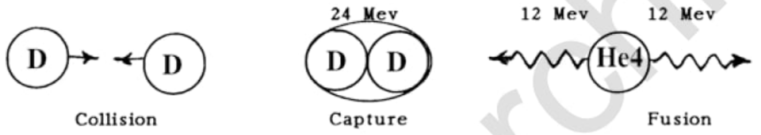

Figure 3a. High velocity DD collision produces He-4 and two 12 MeV gamma rays in center-of-mass coordinates. Because 24 MeV is enough energy to disrupt He-4, the more usual result is that the He-4 immediately decomposes into two deuterons.

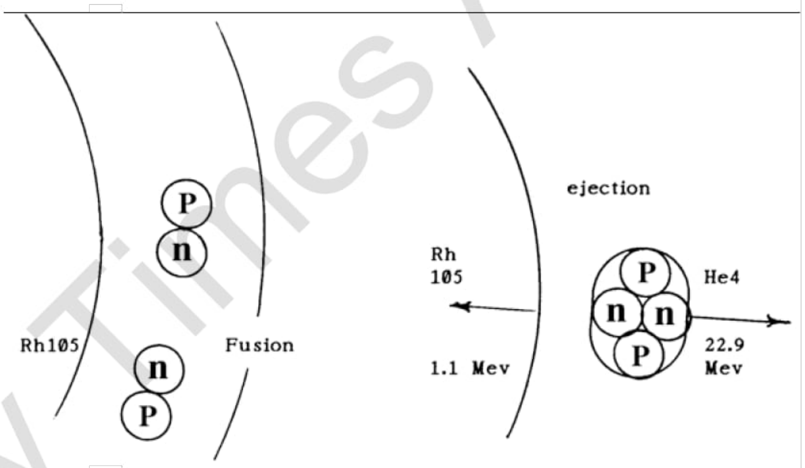

Figure 3b. Capture of two deuterons in the rhodium-catalyzed fusion produces He-4 without gamma rays. Because 22.9 MeV is not enough energy to disrupt He-4, the ejected nucleus is stable.

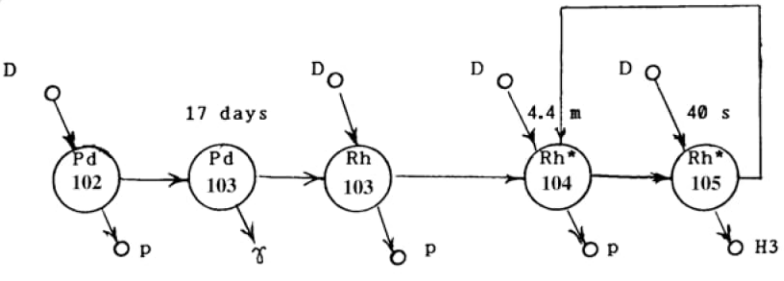

Figure 4a. The rhodium-catalyzed fusion "long loop" in palladium deuteride. The * indicates an excited state.

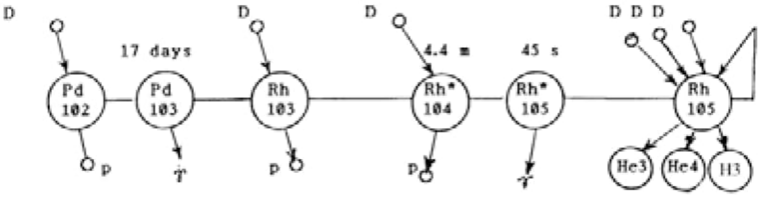

Figure 4b. The rhodium-catalyzed fusion "short loop" in palladium deuteride. The final step involves the capture of either one, two, or three deuterons and results in the production of either H-3, He-3, or He-4.

alyzed fusion can continue for a time which is not dependent on the presence of an electrical current, provided that deuterium is present at the rhodium site and there is no lattice damage which would prevent the deuterium from reaching the rhodium site. The reaction can continue after interruption of the current until either the deuterium is depleted or the Rh decays to Pd or is dislodged from contact with the host metal.

6. The lack of helium and tritium may be the crucial result that disproves the hypotheses presented here, or it may indicate that those products are involved in further nuclear reactions before they are ejected from the Rh composite, as suggested in Reference 5.

No attempt has been made to explain the results of experiments using light water or host metals other than palladium.[9]

Conclusions and Recommendations

Most of the reactions and processes discussed here are well known and are treated in introductory nuclear physics texts such as Reference 3. The only two exceptions are the room temperature deuteron splitting and neutron capture by Pd and Rh and the deuteron capture which leads to the rhodium-catalyzed fusion process. The latter is similar to the carbon-catalyzed proton fusion proposed as a possible nuclear process taking place in the Sun.[3,4]

The most obvious test of the validity of the rhodium-catalyzed fusion hypothesis would be to dope a sample of palladium with a few parts per billion, probably no more than one part per million rhodium. This material would then be used in a Fleischmann-Pons type electrolytic cell, to determine if the reaction rate or rate of heat production could be increased and the reaction made more reliable. A comparison should be made with a cell using ultrapure palladium, as palladium from some sources may have traces of rhodium. One of the first things one should notice would be that the one to six week "charging period" would be eliminated.

If this proves successful, one would want to devise ways of increasing the area-to-volume ratio of the metal to increase the potential rate of deuterium in-diffusion, since that appears to be one of the factors limiting the reaction rate. This would also increase the efficiency of heat removal and help prevent damage to the integrity of the metal. A large area-to-volume ratio would also avoid the possibility of being unable to "turn off" the reaction if there were a large quantity of deuterium stored in the bulk of the metal. Perhaps charging the host metal from the gaseous phase would permit finer, more rapid control. Perhaps a thin coat of Pd doped with Rh on the outside of water-cooling pipes passing through a chamber containing deuterium at high temperature and pressure would be practical. The pipes would have to be made of some material which is impervious to the diffusion of deuterium. A reliable and fail-safe way of controlling the reaction rate is certainly needed. Caution should be used until that is achieved.

A study of how the neutron capture cross section of rhodium varies with temperature should be one of the first projects to be undertaken. Many elements exhibit large increases in cross section, known as resonances, at energies of a few eV. For example, the neutron cross section for indium[3] increases by two orders of magnitude at 1.5 eV, which would be about 15,000°C. This would be a little too hot for home heating, but if Rh-105 should have a resonance near 0.1 eV this could be both an advantage and a hazard. The advantage would be that one could raise the reaction rate by increasing the temperature, which would be desirable for electric power generation. Also, increasing the reaction rate would permit reducing the amount of Rh needed for a given energy output. Considering the price of rhodium, this would be a great benefit. The hazard would be that thermal runaway would be possible; as the metal heats up, the reaction rate increases, as the reaction rate increases, the metal heats up more rapidly. This may be what happened in the Fleischmann-Pons experiment[1] with the 1 cm cube at high current density, what they designated as "ignition."

Except for the high rate of diffusion of hydrogen through palladium at high temperatures, palladium may not be needed in the cathode. Since the proposal is to "dope" the host metal with rhodium, palladium is no longer needed as the parent metal for the rhodium. Other less expensive metals which form hydrides may serve as well, titanium and lead being a couple of examples. As of now, however, palladium appears to be the best suited.

Acknowledgments

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7. Since this paper was first prepared for publication, additional experimental evidence has been published giving evidence that He-4 is produced in sufficient quantities to support the production of heat. See Miles, M.H. *et al*. "Anomalous Effects in Deuterated Systems," *Infinite Energy*, **3**, 15/16.

8. If deuterons are supplied rapidly enough, it is not inconceivable that two or more He-4 nuclei would be formed within the same Rh-105 nuclear well. If so, fusion of the nuclei would occur almost instantaneously in this extremely energetic environment to form beryllium-8, which has a half-life of only 10^{-15} sec. Even so, the process may continue to carbon-12, oxygen-16, etc., accounting for the anomalous isotopes seen in palladium which has been used in heavy water electrolysis experiments.

9. For additional discussion of experimental support offered the Rhodium-Catalyzed Fusion proposal see Josephs, H.C., *Infinite Energy*, **4**, 20, 47.

About the Author

Harold (Hal) Josephs was educated at Midwestern University, Wichita Falls, Texas; the University of Minnesota; and New York University, where he received a Masters of Electrical Engineering degree in 1959. He was employed by Bell Telephone Laboratories, Murray Hill, New Jersey, in the Solid State Device Department for three years. He worked for ten years at Honeywell, Inc. in Minneapolis, Minnesota, in a variety of positions—mostly at the Solid State Electronics Center. He spent the last twenty-one years of his working career as a Professor of Electronics at the Alexandria Technical College, Alexandria, Minnesota. Mr. Josephs has published more than two dozen technical papers and served on technical and political committees. He retired from academia in 1990 and has engaged in cold fusion research since 1989. In 1996 he moved with his wife, Ruby Donahue, to New Mexico where they still reside.

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Anomalous Heat Generated by Electrolysis Using a Palladium Cathode and Heavy Water

Edmund Storms*

Talk given at the American Physical Society (APS) Meeting, Atlanta, Georgia, March 26, 1999

Abstract

Samples of palladium sheet supplied by IMRA Japan were tested for anomalous energy production using electrolysis in heavy water and a sensitive calorimeter. Several samples were found to produce significant power above that being applied to produce electrolysis. This behavior was found to correlate with certain properties of the palladium metal. In addition, the anomalous heat was shown to originate at the cathode.

Introduction

Ten years ago, Professors Pons and Fleischmann claimed to produce excess energy when palladium was electrolyzed in D_2O. Unfortunately, this claim was very difficult to duplicate. The present study was undertaken to understand the nature of palladium which might affect those conditions needed to initiate the claimed anomalous reaction.

It is generally agreed by people in the field that a very high D/Pd ratio is required and this high composition exists in the near surface region. Very few samples of palladium are able to achieve this high composition, much less produce excess energy. If conditions affecting the ability to reach high compositions could be understood, potentially useful palladium could be identified and the claims could be explored with greater success. At least then the reality of the claims could be better evaluated.

Experimental

The basic reason most palladium is not able to achieve high compositions is because the metal expands and forms submicroscopic cracks upon reacting with hydrogen. These cracks provide a pathway for hydrogen loss. Figure 1 shows how palladium expands as hydrogen is added. When the measured volume follows the line, based on the lattice parameter, no cracks are present. However, when the measured volume rises above the line, the presence of internal cracks is indicated. Figure 2 shows a histogram of the amount of excess volume in a collection of palladium samples. As you can see, only a few samples have a small amount of crack formation, a requirement for achieving high surface compositions. Indeed, samples having excess volume above about 3% are essentially useless. It is easy to measure the average composition of a sample. However, because the required composition exists in a thin region near the surface, determining a value for the active region is another matter. Fortunately, the average activity of hydrogen at the surface can be determined by measuring the average open-circuit-voltage (OCV). This quantity is obtained by measuring the voltage between the sample and a stable reference electrode, in this case platinum, while the applied current is interrupted for about one second. Figure 3 shows how the OCV changes with time as a sample reacts with deuterium. Typically, the value rises while deuterium is added to the alpha phase, remains constant while α-phase converts to β-phase, then rises again as deuterium is added to the beta phase. A reverse behavior is seen when current is turned off for an extended time and the contained deuterium is allowed to leave the sample, as shown in Figure 4. Given enough time, all deuterium will leave the metal as D_2 gas.

Only those samples able to achieve an average OCV in excess of 1.2 volts will produce significant excess energy. Figure 5 shows how the average OCV of a sample, which was making excess energy at the time, changed when the applied current was stopped. A phase transition is clearly evident starting at 1.1 V where a new phase converts into β-phase. If this deuterium loss were allowed to continue, another transition would be seen between 0.93 V and 0.97 V where β-PdD begins to convert to α-PdD. This behavior indicates that for excess energy to be produced, β-phase must first convert to another phase having a much higher deuterium activity and composition. Direct measurements

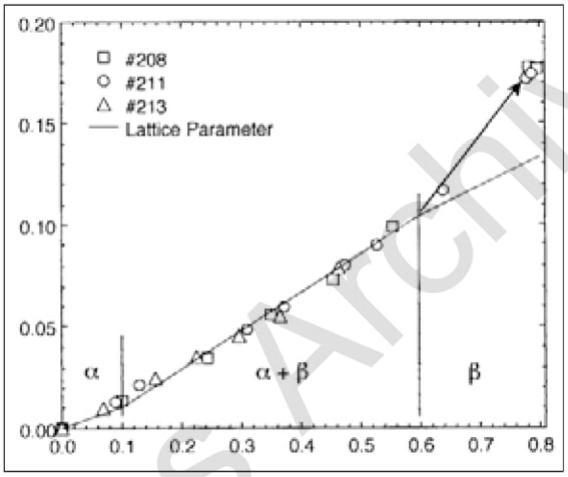

Figure 1. Volume increase caused by hydrogen addition with palladium. Excess volume is seen to increase as hydrogen is added within the beta phase.

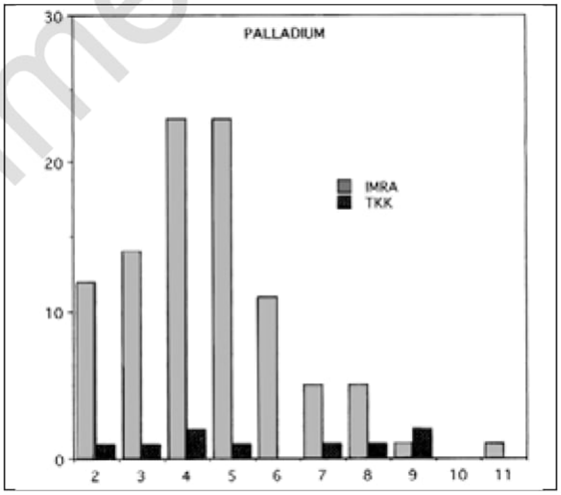

Figure 2. Volume excess over that expected from lattice parameter increase upon reaction with deuterium.

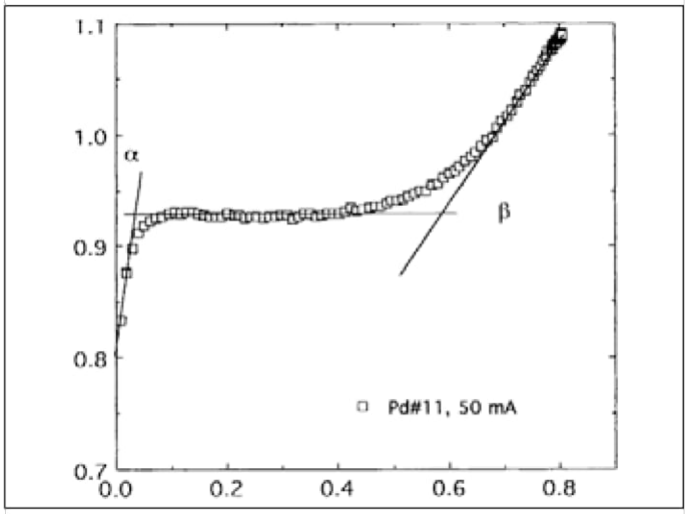

Figure 3. Open circuit voltage relative to platinum during reaction with deuterium during electrolysis.

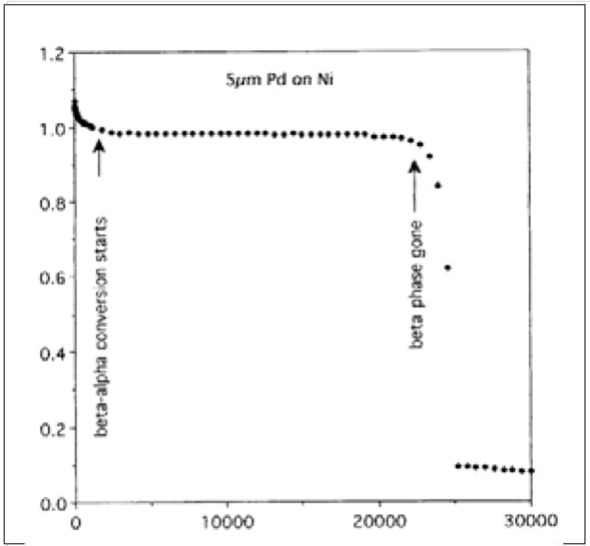

Figure 4. Open circuit voltage measured during deuterium loss from a thin layer of Pd on Ni.

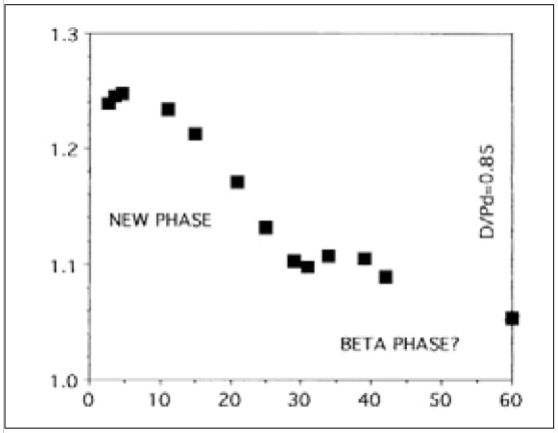

Figure 5. Open circuit voltage during deuterium loss from a sample of palladium which was making excess energy.

of thin films place the composition of this phase above D/Pd=1.5. I propose this phase to be PdD_2.

The calorimeter used to determine excess energy is an isoperibolic-type shown as a cross-section in Figure 6. The cell contains a catalyst to return D_2 and O_2 back to D_2O, the electrolyte is stirred at a constant rate, and the temperature is measured at two levels in the solution and at the cathode using precision thermistors. The amount of heating power is determined by measuring the average temperature across the cell wall and multiplying this value by a previously determined calibration constant. This constant is obtained by applying electrolytic power to the sample after it has been loaded with deuterium but before excess energy is generated. Thus, any claim for excess energy is relative to power produced by the inert sample. Absolute calorimetry is not used. This is important because relative calorimetry removes many errors which skeptics use to reject the measurements.

A plate of palladium (10mm*20mm*1mm) supplied by IMRA (Japan) is loaded at 0.4A and 9°C. The average composition is D/Pd=0.89 with 1% excess volume. After calibration, the current is periodically stepped from 0.1A to 3A. Figure 7 shows the excess power as a function of applied current at various times. After nineteen hours of electrolysis, no sign of excess energy is visible. A small amount of excess power is apparent after sixty-three hours and this excess increases until 4.5 watts is seen at 3A after 196 hours. A clear onset for the effect is seen at 1 amp. A similar onset has been seen in all previous studies by other workers when such measurements were made.

The deloading behavior of the OCV, shown previously as Figure 4, was obtained between data sets No. 6 and No. 7. This same general behavior was also seen several times after the sample had been repeatedly deloaded and then reloaded within the calorimeter. In other words, the sample showed reproducible behavior.

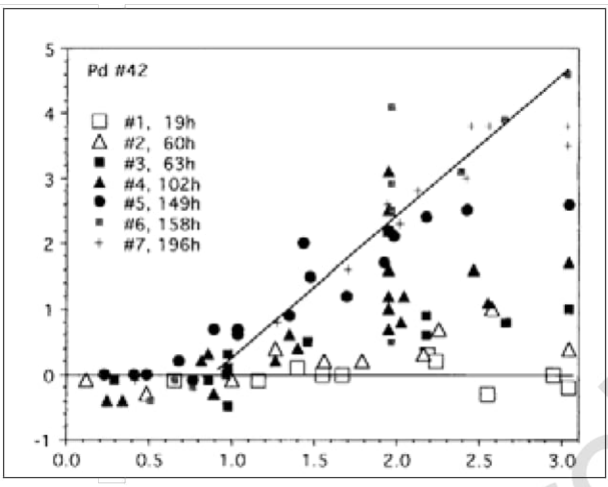

Figure 7. Excess power as a function of applied current after the indicated times of electrolysis at 0.1A.

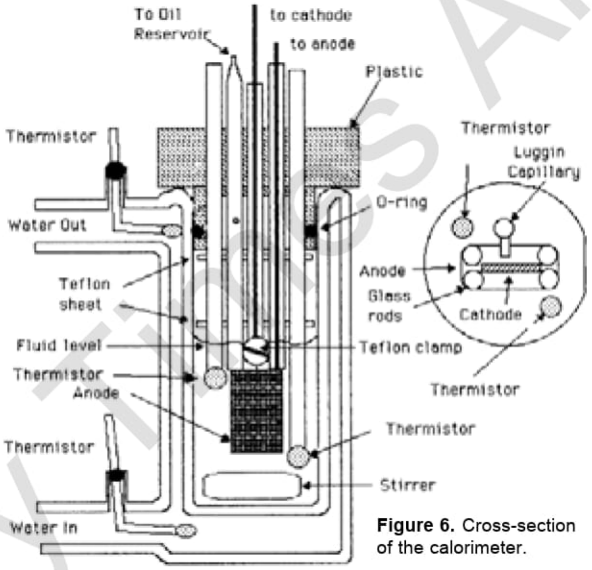

Figure 6. Cross-section of the calorimeter.

Ninety samples were studied during this work. Of these, only five samples were found to have the ability to achieve a high average OCV and exhibit excess power. Clearly, the lack of reproducibility using the Pons-Fleischmann method is caused by a materials problem, not because of error or sloppy work.

Conclusion

The initial claims for excess energy production made by Pons and Fleischmann were prematurely rejected. Continued worldwide work shows evidence for excess energy production using nine different methods, several different materials, and five different styles of calorimetry. The work presented here shows that the difficulty in reproducing the Pons-Fleischmann method is caused in part by the nature of palladium. Suitable palladium produces the effect without difficulty. This experience has been shared by those workers who have studied a large number of samples, including Dr. Mike McKubre (SRI), Dr. Mel Miles (Naval Weapons Laboratory), and Professors Pons and Fleischmann (IMRA Europe).

About the Author

Edmund Storms obtained a Ph.D. in radiochemistry from Washington University (St. Louis, Missouri) and recently retired from the Los Alamos National Laboratory after thirty-four years of service. His work there involved basic research in the field of high temperature chemistry as applied to materials used in nuclear power and propulsion reactors, including studies of the "cold fusion" effect. Over seventy reviewed publications and monographs resulted from this work as well as several books, all describing an assortment of material properties. He presently lives in Santa Fe where he is investigating the "cold fusion" effect in his own laboratory. These studies have resulted in fourteen publications, including three complete scientific reviews of the field, one published in 1991, another in 1996, and the final one in 1998. In May 1993, he was invited to testify before a congressional committee about the "cold fusion" effect.

*Energy K. Systems, 2140 Paseo Ponderosa, Santa Fe, New Mexico 87501

The Status of Dr. Ruggero Maria Santilli's Complaints

Dr. Eugene F. Mallove, Editor-in-Chief

Readers of recent issues of *Infinite Energy* are aware that we had been in the process of publishing a series of Dr. Ruggero Santilli's technical articles under the general banner, "Physical Laws of the Emerging New Energies as Predicted by Hadronic Mechanics." Dr. Santilli stated his affiliation as the Institute for Basic Research of Palm Harbor, Florida, an organization that he founded many years ago. The issues in which the articles appeared and the subtopics of the articles were:

- *Infinite Energy*, No. 22, 1998 (Oct./Nov.), *I. Insufficiencies of Quantum Mechanics*, 33-49.
- *Infinite Energy*, No. 23, 1999 (Jan./Feb.), *II. The New Mechanics (Intro)*, 69-71.
- *Infinite Energy*, No. 25, 1999 (May/June), *II-B. The New Mechanics*, 60-74; and *III-A. Structure of the Neutron and New Energies of Class I*, 75-85.

In addition to these articles, in *Infinite Energy*, No. 24, we published an exchange of rather hostile letters between Dr. Santilli and Dr. Elio Conte of Italy (pp. 56-57)—Santilli's was submitted first. This exchange dealt with charges by Dr. Santilli that Dr. Conte, in his article "An Experiment Indicates the Nuclear Fusion of the Proton and Electron into a Neutron" (co-authored by Maria Pieralice, *Infinite Energy*, No. 23, 67-68) had: (1) Not properly cited earlier experimentalists, Don Borghi *et al.*, who had claimed to demonstrate the fusion of the electron and proton into a neutron, and (2) not cited Santilli's work and mentioned its alleged more perfect ability to represent physical reality, neutron structure in particular. Dr. Conte attacked these accusations and answered them.

Dr. Santilli was apparently very happy with this *Infinite Energy* No. 24 (March/April 1999—the tenth anniversary of cold fusion issue). On March 21, 1999, he sent me the following e-mail:

Subject: Congrats & Thanks
Sent: 3/21/19 4:33 PM
Received: 3/21/99 10:47 AM
From: Institute for Basic Research, ibr@gte.net
To: Inf. Energy: E. Mallove, editor@infinite-energy.com

Dr Eugene Mallove
Editor in Chief
Infinite Energy

Dear Eugene,

1) Congratulations for the beautiful special issue of IE on the 100-th *[sic]* anniversary of cold fusion. It is truly valuable.
2) Special compliments for your firm stand on scientific values in the editorial.
3) Thanks for publishing my unmasking of Conte's little and miopic *[sic]* scheme.
You and your staff did an excellent Editorial job and it shows.
I shall continue to back you up in any possible way I can.

Regards
Ruggero

We were to learn shortly how much Dr. Santilli would "back us up." As reported in our last issue (*Infinite Energy*, No. 26, 8) on May 31, 1999, we received correspondence from a "Mr. William Pound" of the Institute for Basic Research in which he stated that Dr. Conte had plagiarized Dr. Santilli's work on the pages of *Infinite Energy* No. 24 in the article, "Theoretical Indications of the Possibility of Nuclear Reactions at Low Energy." So for the very issue of *Infinite Energy* in which Santilli-Pound were *two-months later* to find an alleged "obvious" plagiarism, we have enthusiastic praise for the whole issue. If the plagiarism was so "obvious," why was it not noticed in March?

To continue this narrative, we shall simply repeat in part what we wrote in No. 26 about this important developing accusation, which we took with utmost seriousness:

"Curiously, Issue No. 24 had been in the hands of Dr. Santilli and Pound since approximately March 21, 1999—for almost two months. The delayed discovery of alleged plagiarism occurred apparently at the same time that Santilli became angry about a letter by Mr. Ken Rauen in Issue No. 25 in which both Santilli and the allegedly offending scientist were taken to task for their earlier published squabbles about proper citation—see exchange of letters, Conte vs. Santilli, in No. 24.

"We had every intention, at the time of the Santilli-Pound plagiarism accusation, of continuing to publish Dr. Santilli's series, specifically section III-B was to have appeared in this issue (No. 26). We had already edited this material in preparation for publication. For some reason Dr. Santilli became enraged that we were not able to publish his multi-page denunciation of the other scientist in this current issue before we had even finished our investigation of the Santilli-Pound charges....

"We note that on May 31, Pound-Santilli strongly suggested that Dr. Conte not be given the opportunity to view the charges against him for rebuttal in the issue in which the denunciation appeared. This is the specific text:

"3) You abstain from sending my condemnation to Conte for his comments. If you instead elect to send it to him, you will suffer additional credibility damage. In the pursuit of an illusory democracy, you will end up publishing manifest trash, lies and junk science as that in Conte's replay [*sic*] to Ruggero, such as the lie that don [*sic*] Borghi and his team did not look for the cold fusion of protons and electron into neutrons, or the ridiculous statement that Ruggero condemned Conte because he is envious of him (!!!), and other statements so manifestly false that can only cause additional damage to you and to your important journal.

"Subsequently, on June 9, we received a fax from Dr. Santilli in which he formally withdrew the remainder of the series of technical articles. We accepted this withdrawal with regret, based on a continuing series of preposterous legal threats against this magazine by Dr. Santilli. These threats have now (June 2, 1999) escalated to this warning: 'If I receive no notification from you ON OR BEFORE JULY 7, 1999, you are hereby formally notified that I shall assume that you persists [*sic*] in your illegal and profoundly immoral stand of stubbornly refusing rapid corrections for a serious editorial tort, and I shall feel free to file a lawsuit from July 8, 1999, on[*sic]* without any waiting for your next issue. Moreover, once filed, the lawsuit will stand even to the eventuality that you subsequently publish our denunciation. . .' "

Most regrettably, especially for him, on July 16 Dr. Santilli acted *pro se* (without legal counsel) and filed a lawsuit against *Infinite Energy* (owned by New Hampshire corporation Cold Fusion Technology, Inc.) and many other individuals. The lead defendant is Dr. Elio Conte. The baseless suit against this magazine and its staff members, Dr. Mallove and Managing Editor Barbara DelloRusso, was filed in U.S. District Court, Middle District of Florida, Tampa Division, Case Number 991675C1UT25A. (Anyone can review this suit at the court, 801 N. Florida Ave, Tampa,. FL 33602, Tel. 813-301-5400). The suit is forty-one single-spaced pages organized into some 252 paragraphs. This is the complete list of eleven defendants:

- Elio Conte
    Libero Istituto Universitario Internazionale
    (Bari, Italy)
- Maria Pieralice
    Libero Istituto Universitario Internazionale
    (Bari, Italy)
- The President
    Libero Istituto Universitario Internazionale
    (Bari, Italy)
- Eugene E. Mallove [Note: Santilli erroneously used E. rather than F. for the middle initial]
- Barbara A.F. DelloRusso
- Thomas E. Phipps, Jr.
- Cold Fusion Technology, Inc.
- Enrico Panarella
    ALFT, Inc., Hull, Canada
- K. Charbonneau
    ALFT, Inc., Hull, Canada
- B.R. Robinson
    ALFT, Inc., Hull, Canada
- ALFT, Inc., Hull, Canada

The lawsuit has been posted on Santilli's special web site that has been set up under the banner, "Committee on Scientific Ethics"—a "committee," one might add, that has no listed members except the implied member, Ruggero Santilli (http://home1.science2.gte.net). Santilli lists one suit, "Lawsuit #1" that was carried out by him apparently against two individuals at the First University of Rome, Italy in 1995. The text for this lawsuit is in Italian. "Lawsuit #2" is the one against Conte *et al*. The site has ominous notation for further webmaster work designating: "Lawsuit #3 In preparation"; "Lawsuit #4 In preparation"; and "Lawsuit #5 In preparation." One wonders whom Santilli will be attacking next. He has already threatened to sue numerous individuals.

Our exploration has now uncovered evidence of other threats and admonitions against scientists and meeting organizers by Dr. Santilli—all revolving around the issue of proper citation of *his* work. In fact, on this "Committee on Scientific Ethics" web site there are nine (!) highly detailed "Fair Warnings" about what kinds of mathematical formalisms and physics developed by Santilli, which if not cited, will carry the possibility of legal action—including *criminal* complaints!

In Lawsuit #2, Dr. Santilli may *claim* to have a legal case against Dr. Conte—though in our view Conte has put up a credible defense against the Santilli accusations of plagiarism (see Conte's article in this issue). But how did all these other names and organizations get lumped into this suit? What evidence does Santilli have in his suit's formal Claim: "plagiarisms by all Defendants, whether acting individually or in a collegial conspiratorial way, of copyrighted publications by the Plaintiff." What is his "evidence of the organized conspiratorial character of the action by all defendants." Conspiracy? This is pure hogwash.

Infinite Energy readers may be properly amazed how noted frontier physics experimenter, theorist, and heretic Dr. Thomas Phipps, Jr. came to be a defendant. Santilli had called Dr. Phipps on the telephone and was raging against Dr. Mallove in a most abusive and personal way, according to Dr. Phipps. Phipps then submitted a measured letter to Santilli in which he cautioned him against attacking one of the foremost supporters of openness to new ideas in physics, Mallove at *Infinite Energy*. At the same time Phipps formally resigned his honorary position on the Institute for Basic Research's roster of honorees. That letter provoked Santilli to write a blistering response (via e-mail), which is filled with invective against Phipps.

The Canadian defendants being sued are associated with *Physics Essays*, a fine sister publication in frontier physics, but with more exclusively highly technical content and no specific focus on energy. They will mount their own defense against Santilli. Mallove, DelloRusso, Cold Fusion Technology, Inc., and Phipps are being defended by an excellent law firm in Florida—Shear, Newman, Rosenkranz, Burton, & Lamb, P.A. The cost of the defense, though neither appreciated nor expected by us, will be worth it, if, as we anticipate, this suit is either dismissed outright for many obvious reasons or if it leads to Dr. Santilli and his IBR being admonished not to make *legal* issues about non-citation when *distinct* physics theories evolve from related mathematical structures, such as Dr. Conte's perhaps has.

Most bizarre was an e-mail received from Santilli's IBR on July 21, 1999, signed by William F. Pound:

> YOU ARE AGAIN PROHIBITED TO PUBLISH MY DENUNCIATION OF SCIENTIFIC FRAUD OF HADRONIC MECHANICS BY CONTE AT YOUR MAGAZINE, AS SUBMITTED TO YOU ON MAY 31, 1999. THIS PROHIBITION IS DUE TO THE RECENT FILING OF LEGAL ACTIONS AGAINST YOU AND YOUR GROUP BY PROF. SANTILLI.
>
> IN THE EVENT YOU FAIL TO OBEY THIS PROHIBITION, I SHALL PERSONALLY SUE YOU AND YOUR FRIENDS. THE SOLE THING YOU CAN DO IN LIEU OF MY DENUNCIATION IS TO PUBLISH EXCERPTS FROM THE LAW SUIT AGAINST YOU. YOU SHOULD HAVE BEEN SERVED BY THE SHERIFF IN CONCORD BY NOW. IN THE EVENT OF DELAY, THE LAW SUIT IS AVAILABLE IN ITS INTEGRITY[*sic*] IN THE WEB SITE
>
> http://home1.gte.net/science2/
>
> ANY ADDITIONAL DISTORTIONS-MISREPRESENTATIONS-DECEPTIONS IN EXCERPTING THE LAW SUITS WILL BE FILED AS FURTHER CLAIMS.
>
> W. F. POUND

IBR is saying in so many words: **"I will bring suit against you if you publish the 'denunciation of Conte'—for which I am currently suing you for *not* publishing as fast as I demanded that you publish it."**

We are happy to oblige Dr. Santilli here and not publish the Pound-Santilli "denunciation," even though we would have every right to do so. There is no point in *Infinite Energy* readers having to wade through such a morass. Besides, the essence of Santilli's lengthy complaints are right on the "Committee for Scientific Ethics" site—in all 252 paragraphs. Dr. Conte's answer to Santilli's accusations, in the form of an account of how he developed his theory, will suffice as a response. Readers of *Infinite Energy* can examine both documents and the surrounding facts that we have presented and make up their minds about who is being reasonable.

We understand that our friend and publishing colleague Hal Fox, Editor of the *Journal of New Energy*, will be continuing the publication of the scientific series by Dr. Santilli that began in *Infinite Energy*. We wish Hal well and are glad that the series will be completed elsewhere.

Given Santilli's apparent litigious intentions no one panelist could have effectively served as a referee in this debate. Now that Santilli is making further threats and has acted against his one-time friends, no formal determination by us of the merits of his plagiarism charge is either warranted or possible. On the other hand, if the *substance* of this case ever goes to trial the record of Santilli's threats, intimidation, and paradoxical demands will be absolutely clear. We trust it is obvious to our readers who is wasting time, energy, and resources that this field can ill afford to squander.

> *"It's nice to be important,*
> *but it's important to be nice."*
>
> *Submited by Walter S. Rosenthal*

A Review of Biquaternion Quantum Mechanics

Elio Conte*

Introduction

When an author develops a new theory, he also has the professional duty to give an adequate exposition. There is also the question of how long a satisfactory statement of the theory should be.

This is also true when the author is convinced that his previous expositions of the theory only provide just the amount of information required for a careful and professional reader to fully understand the basic statements of the enunciated theory. This is precisely the aim that moves me in adding some further comments to my previous articles that appeared in *Infinite Energy.*[1]

The Biquaternion Quantum Mechanics (BQM)

BQM uses two terms, "biquaternion" and "quantum mechanics." Biquaternions derive from the quaternions that were introduced in mathematics by Hamilton.[2] R. Fueter founded the theory of functions of a quaternion variable[3] in 1934.

I became aware of biquaternions around 1980 after reading the articles of K. Imaeda[4] which appeared in 1976 and 1979, respectively. After this date we started to introduce biquaternions into quantum mechanics.[5] K. Imaeda introduced biquaternions. My papers clearly show that we provided a proof of such numbers, also characterizing their algebra. A brief discussion of this algebra is given in *Infinite Energy*[1] with formulas (1.1) to (1.18).

An important formula is (1.15): It was introduced in the biquaternions in 1975 by Imaeda[4] (see his formulas, p. 142), and it was discussed in detail in 1979 by Imaeda (see paragraph 2.2 from pp. 276-282). In this paper we have a detailed exposition of the Linear Homogeneous Biquaternion Transformations (LHBT) that I will use in BQM as is clearly indicated in all my papers and in particular in *Infinite Energy.*[1] It must be explicitly said that K. Imaeda used LHBT for a new formulation of classical electrodynamics and for a quaternionic formulation of tachyons, super luminal transformations, and a complex space-time.

This first point must be clear. Readers must fix in their minds what I say here: ***I have used only biquaternions and Linear Homogeneous Biquaternion Transformations*** (LHBT) as considered by Imaeda. Let us follow the development of BQM on the basis of this. We introduced biquaternions and LHBT in quantum mechanics.

Quantum mechanics (QM) is the second term of BQM. When in 1966 I started to study QM, I was astonished by the fact that Heisenberg had obtained such new and important knowledge about the nature of physical reality at the quantum level only using, at the beginning of his activity of research, simple matrix algebra. I thought about how matrix algebra gave Heisenberg the opportunity to introduce the innovative and radical principle of indeterminism into nature, and I was convinced that a field of numbers could also be useful in expressing the same principle. In the summer of 1970, I became aware of the existing quaternions and was persuaded of the following: a profound link should exist between quantum mechanics and quaternions. This was the origin of BQM.

As a young physicist, I started to expose my idea with contributions and congresses,[6] in particular in 1974 at the Balkanski Mathematicki Kongres in Beograd through 1996 with an article published in *Physics Essays*[7] containing in detail and in a sophisticated manner all ***the principles and the basic rules of BQM***. I say that this article contains the rules of BQM in a "sophisticated" manner, because there is actually another way to treat BQM in detail, simplifying all the procedures. Following along this line, one appreciates BQM much more. There is an excellent book by T.J. Jordan[8] entitled, *Quantum Mechanics in Simple Matrix Form*. This book was written to make understanding QM easy, but it is an excellent text for me.

Jordan uses only matrices to explain QM but one may read the book, substituting for the matrices. The biquaternion basic elements that I introduce, for example, in *Infinite Energy* from (1.1) to (1.18) and with particular attention to (1.17) to (1.18) and to my statement on page 50 where I say that "we may find a matrix representation of biquaternions at any order." In this way Jordan's book becomes an excellent source for BQM as also discussed in detail in my paper entitled, "On the Rules of Biquaternion Quantum Mechanics."[9] Following Jordan's book, one arrives at the following general statement that, as said previously, was my first persuasion in 1970: ***All QM is only a biquaternion calculus.*** There is not a chapter of the usual quantum mechanics (from the hydrogen atom to the invariance principles) that cannot be expressed by the biquaternion calculus. ***All the usual quantum mechanics is biquaternion calculus made by the simple rules of the algebra of such hyper complex numbers.***

The basic elements e_i (i=1, 2, 3) of biquaternions may be used not only to represent the spin or the magnetic moment of a particle but also to represent any other quantity that is of interest in ordinary quantum mechanics.

This is the most important statement of BQM (I must repeat this statement several times because it is of fundamental importance): As shown by Jordan and in my paragraph on the ***basic rules of BQM*** in (7), ***any quantity of quantum mechanics is realized starting with biquaternions e_i (i=1, 2, 3), and thus it is in the algebra of these numbers*** wherein lies the key to all the extraordinary properties exhibited in the quantum theory. The Jordan book and References 5 and 7 make unequivocally clear this basic statement of ours.

According to Jordan, there is still another motivation to appreciate BQM. Let us consider a general biquaternion, given as an example in (1.9)[1] with $z_0=0$. It is shown unequivocally that Z^2 is always a real number. Thus, when we consider Z as representing a quantum quantity, we have it that Z^2 is, for example, a real number α. Z being a matrix, we may say that Z may assume the possible values ±α in a given state of a fixed quantum system. Thus we have that $-\alpha \leq \langle Z \rangle \leq +\alpha$, and calling $p(-\alpha)$ the probability for Z to assume the value -α and calling $p(+\alpha)$ the probability for Z to assume the value +α, we have

$$p(-\alpha) + p(\alpha) = 1 \tag{1}$$

with

$$\langle Z \rangle = (-\alpha)\, p(-\alpha) + (\alpha)\, p(\alpha) \tag{2}$$

and thus

$$p(-\alpha) = \frac{1}{2} - \frac{\langle Z \rangle}{2\alpha}$$

$$p(+\alpha) = \frac{1}{2} + \frac{\langle Z \rangle}{2\alpha}. \tag{3}$$

In this manner we arrive at the second important statement of BQM. ***Quantum systems may be analyzed by BQM with the simple rules as indicated now in (1), (2) and (3) without the need to introduce at any cost the wave functions that are usually used in QM.*** We have, in conclusion, the biquaternion calculus of QM which shows itself as a slender and more versatile structure with respect to the usual quantum theory.

This is the BQM: a biquaternion calculus of QM that at all points is in accord with the standard quantum theory and, in particular, BQM shows in detail that all the extraordinary properties that are exhibited by quantum systems in nature originate in particular in the intrinsic properties exhibited from the algebra of biquaternions. For example, as we know, an extraordinary property of the quantum systems is the well-known nonlocality. In Reference 10 you can find excellent proof of the nonlocality of quantum systems only using biquaternions.

After this discussion, I hope I have convinced the reader that BQM has reached its final formulation by way of an absolutely independent elaboration, the only aim of which was to reproduce step-by-step all the usual quantum mechanics and to confirm that it may be rederived using only the simple rules of the algebra of the employed hyper complex numbers. In particular, Heisenberg's indetermination principle also is an expression of the noncommutative character of the multiplication of biquaternions.

Let us consider two biquaternions expressing Q and P [9]

$$Q = x_0 + x_1e_1 + x_2e_2 + x_3e_3 \qquad (4)$$
$$P = y_0 + y_1e_1 + y_2e_2 + y_3e_3$$

with x_i and y_i (i=0, 1, 2, 3) real numbers. We have that

$$QP - PQ = b_1e_1 + b_2e_2 + b_3e_3 = T \qquad (5)$$

with

$$b_1 = 2i(y_3x_2 - y_2x_3)\,; \quad b_2 = 2i(y_1x_3 - y_3x_1)\,; \quad b_3 = 2i(y_2x_1 - y_1x_2).$$

Let us now apply the basic rule of BQM to find the numerical value that a given quantity (represented by a biquaternion) may assume. We have that

$$T^2 = b_1^2 + b_2^2 + b_3^2 = -k^2 \qquad (6)$$

T may assume the values ±ik. Heisenberg's well-known relations give $i\hbar$ with $k = \hbar$.

I give still another interesting detail. It is useful to understand that Schrödinger's and Dirac's equations are linked by the same biquaternion approach. Let us remember that in deriving his equation, Dirac had the problem of finding the following square root[9]

$$(p_x^2 + p_y^2 + p_z^2 + m^2)^{1/2} \qquad (7)$$

This is a biquaternion. We have

$$H_D = (p_x^2 + p_y^2 + p_z^2 + m^2)^{1/2} = p_xe_1 + p_ye_2 + p_ze_3 + \beta\, m = [(p_xe_1 + p_ye_2 + p_ze_3 + \beta\, m)]^{1/2.} \qquad (8)$$

Schrödinger's equation has hamiltonian H_s linked to H_D.[9]

$$H_s = (p_xe_1 + p_ye_2 + p_ze_3)\,(p_xe_1 + p_ye_2 + p_ze_3) \qquad (9)$$

with

$$H_D = (H_s + \beta^2\, m^2)^{1/2}.$$

This statement covers the first part of my exposition on BQM. At this point I expect the reader to pose the following question. I understand your interest in introducing BQM. Some points are interesting, for example, when it shows that all of QM is a biquaternion calculus, or when it establishes a profound link existing between the algebra of biquaternions and all the extraordinary properties exhibited in nature from quantum systems, such as the indeterminism, non-locality, and so on. But is it all here? Is there some other reason to consider the importance of BQM?

In the answer to these questions I have all the reasons for this further comment. To understand it fully, I must return to reconsider the algebra of biquaternions and one of the basic statements that I introduced for BQM. Let us start with this statement:

In the previous pages I outlined that, according to the book by Jordan[8] (see in particular chapters 9, 10, and 11) and my papers, References 5 and 9, ***any quantum quantity of BQM starts with the basic elements e_i (i=1, 2, 3) of biquaternions and/or is realized by the basic elements of biquaternions. I have no other quantities except the basic elements of biquaternions or other quantities realized starting with these basic elements.*** This is the first point.

The next point is represented from (1.15) of my paper in *Infinite Energy*.[1] Let me explain. In the previous pages I outlined in detail that biquaternions are transformed by transformations and, in particular, by LHBT as discussed in detail by K. Imaeda. It seems expected that, considering the BQM, one should ask himself what happens to his BQM theory when he applies LHBT to e_i (i=1, 2, 3) and to complete biquaternions. It seems also expected that when one decides to examine what happens to his BQM theory when he uses LHBT, he should decide to start his study by considering a LHBT of the following form

$$e_i'(\lambda) = U(\lambda)\, e_i\, U^{+}(\lambda) \qquad (10)$$

with U(λ) and U⁺(λ) biquaternions. The reason to select this particular kind of transformation cannot be asked of a physicist, and particularly of a physicist active in QM, since all the researchers in the world know that this is precisely the most interesting, the most appropriate, and the most important transformation that correspondingly acts in the usual QM.

I hope that I have laid to rest another important point of discussion. I had considered, at the initial stage of my studies, that the LHBT in (10) applied to biquaternions and only to biquaternions, for the evident reason that I proceed with a quantum mechanics realized only with biquaternions, and the particular kind of transformation (10) is of particular importance in the usual QM. As well, when I considered Heisenberg's principle I used (5) with biquaternions Q and P but preserved the original form of calculation used in the standard QM, (that is to say . . . QP - PQ = . . .). In the same manner, deciding to use a LHBT for biquaternions, I have used the same kind of calculation with U and U⁺ also used this time in the standard QM. In order to support the choice used in (10), and this could constitute the most controversial point of our discussion, I invite any reader not only to remember how frequently it was used by physicists active in QM but, in particular, to verify how frequently it was employed, for example, by Jordan just in the case of the matrices e_i. For example, chapter 23 and page 189 or pages 190, 191, 195, and so on. No doubt may exist that (10) is currently employed in QM.

Let us now go to the third point. Using biquaternions to represent (10), one verifies immediately that he has two possibilities: U U⁺ = 1 or U U⁺ = I(λ). This may be a controversial point, so I will clarify.

Is there any physicist using (10) who could have avoided verifying and examining the case U U⁺ = 1? I think there is no physicist able to ignore the verification and the consistency of the standard QM and BQM in the case U U⁺ = 1. For example, one may read the book by T.J. Jordan to verify in how many forms this author has used this transformation in the general case of his treatment.

Is there any physicist who, after considering the case U U⁺ = 1, could have eliminated his interest to examine also the case U(λ) U⁺(λ) = I(λ)? This physicist should never be informed about the possibility of hidden variables in quantum theories.

So, we arrive at the point previously considered: what is the reason why BQM may be so important? These are the points:

- I have said previously that all the BQM is realized by starting with the basic elements of biquaternions. Consequently, if, using (10), I show that, by using the LHBT in (10), I may transform e_i in $e_i(\lambda)$, immediately I deduce that this may be an operation ***to generalize*** the basic elements of biquaternions, to generalize ***BQM*** and, consequently, to ***generalize all the usual QM.***
- Since the usual QM has spoken on the possible existence of hidden variables for about fifty years, I may also be able to understand that such hidden variables could enter in the theory just as in (10), and so I can formulate this possible model of BQM showing that there is the standard BQM that

reproduces all the results of the usual QM, and, in addition, it may be a BQM that might substantially be a possible ***generalization*** of the standard theory and containing $\lambda = \lambda(\ldots)$ as possible hidden variables.

- There is still another point that I show. Since e_i (i=1, 2, 3) is simply $e_i'(\lambda)$ with $\lambda = 1$, I arrive at the conclusion that the usual QM is just a theory containing hidden variables—particularly, this is the case of the theory that contains hidden variables with values $\lambda = 1$—and the other formulations of BQM, containing hidden variables $\lambda \neq 1$, represent a possible and very interesting generalization of the standard case. I think also that in this possible case, I find myself able to open again the debate on the foundations of quantum mechanics and that BQM is actually a new formulation promising very new and innovative results.

So, we have arrived at the conclusion of this section. We have seen how BQM was born; we have seen it starting with the very simple idea of executing some calculations of quantum mechanics using biquaternions. We have arrived at the point of a very general quantum theory, the BQM, whose importance is now clear. These are, of course, the basic statements and ideas that I used. Can anyone show that these points of BQM, previously indicated in detail, were reached by some other author? Did I, in my formulation, use some concept or information that was not previously contained in the usual textbooks of quantum mechanics?

On Some Possible Applications of BQM: The Case of the Neutron

It is time to consider some possible applications of the BQM and, in particular, the case of the formation of the neutron that I discussed in *Infinite Energy*.[1]

Let us start by discussing the first part of the article in *Infinite Energy* from the second column of page 50 to the first column of page 51 where it stops with the words "well-known from nuclear physics." As a preliminary, we will discuss these contents.

I used the first nine lines to show that I would use BQM previously published elsewhere, and that I would use LHBT to get the quantum mechanical generalization that is needed and in line 101 concluded, "Let us ***summarize*** the results"[2]: ***It is intended "about BQM exposed in detail in previous publications reported in reference.***" In fact, in Reference 2 one finds the indication of some of my previous papers, and it is intended that they are necessary to fully understand the article in *Infinite Energy.*

Let us consider now that about May 1994 I received from R.M. Santilli[11] the preprint (I think) of a book entitled *Elements of Hadronic Mechanics* constituting three volumes: *Elements of Hadronic Mechanics, Volume I: Mathematical Foundations*; *Volume II: Theoretical Foundations*; and *Volume III: Experimental Verifications*. In Chapter three of Volume III, with regard to the neutron, one finds a section entitled, "A General Statement on the Problem," from page 86 to 97, and an article entitled, "Application of Isosymmetries to the Apparent Chemical Synthesis of Hadrons," from page 99 to 176. The author of the article is R.M. Santilli, and in front it says ***submitted for publication*** without any other information. From page 192 to 211 is another article entitled, "Apparent Consistency of Rutherford's Hypothesis of the Neutron as Compressed Hydrogen Atom," author R.M. Santilli. It is declared that it appeared in *Hadronic Journal*, **13**, 513-531, 1990. We find also another article, "Isorelativistic Treatment of Rutherford's Neutron as Compressed Hydrogen Atom" by A.O.E. Animalu and Santilli, with the following note: "Draft prepared by Animalu to be finalized by Santilli and submitted for publication." It goes from page 212 to 240. I believe no other articles regarding the neutron are included in Volume III except the reprint of the experiment of Don Borghi, C. Giori, and A. Dall'Olio. As I remember, I also had found some other articles[11] with different basic information and obviously such quoted papers are at the disposal of anyone to verify what I will now discuss.

Let us return now to my article: I started, from line 11 to line 14, to select a proper biquaternion U to effect my LHBT in accord with my project of BQM. In (2.1) I generalize (by using this selected U and thus U⁺) the basic commutator of quantum mechanics. Note that, in line 10, I recalled my previous results on BQM in (2). I generalize now such basic commutator. In Reference 2 of that article I recall my previous article entitled, "A Generalization of Schrödinger's Equation Using Biquaternions: The p ossibility of Fusion of Particles," which appeared in Volume 8, Number 1, 1995 of *Physics Essays*. In this article the first two pages are totally devoted to my ideas on BQM (previously exposed in the first section of this discussion) and, on this basis, I came to consider the same possible generalization of a basic commutator in quantum mechanics. This happens precisely in (21) where I consider in detail the possible generalization of the BQM by LHBT as discussed also in the previous section.

Was I inspired from previous papers to consider a possible generalization of the basic commutator in QM by using an LHBT? The answer is negative. I was in some manner supported in the possibility of generalizing the QM basic commutator from two previous excellent suggestions of important authors. The first was T. Kaneno, who in 1960 published an article[12] entitled "On a Possible Generalization of Quantum Mechanics" which appeared in *Progress of Theoretical Physics*. Read (3.17) on page 27 and you will find that for the first time it was suggested that a basic QM commutator containing $\hbar S$ instead of $\hbar$ only. There is more. T. Kaneno correctly mentions that he previously considered P.M. Dirac (see Reference 3 in the paper by Kaneno) for a comparison of the commutation relations and the classical Poisson brackets, obtaining the basic commutator written as

$$q_s\, p_a - p_a\, q_s = h\, \delta_{as} \vartheta;$$

ϑ being a normalized pure imaginary quaternion. For details see (45), (46), (48), (49), (50) of my paper in *Physics Essays*.

Thus, as early as 1960 the paper by Kaneno suggested to me the possibility of considering a generalization of the basic QM commutator using biquaternions (note that Kaneno obtains, in fact, a ***pure imaginary quaternion*** that is equivalent to a ***biquaternion***), and also to consider a possible generalization of the basic quantum action passing from $\hbar$ to $\hbar\vartheta$. This gives the generalization of the quantum action A' given in line 24 of the paper that we are considering and published in *Infinite Energy*. We must add still other information:

1) I quoted the paper of T. Kaneno in my paper in *Physics Essays* (see Reference 3);
2) I employed a lot of arguments (in my paper in *Physics Essays*) to explain the physical meaning of $\hbar S$ instead of $\hbar$ also recalling the contributions of various authors who previously suggested a possible fluctuation of the quantum action in QM employing biquaternions or quaternions.
3) Formulating Schrödinger's generalized equation in *Physics Essays* and *Infinite Energy*, I used an explicit expression for the action and, precisely, the expression introduced by Janussis *et al.* as it was explicitly indicated in Reference 6 of *Physics Essays* and recalling (2) in *Infinite Energy*.
4) In my paper in *Physics Essays* for Schrödinger's equation I showed the Hamiltonian to be the norm of a biquaternion. To verify, see in particular (22), (23), (24), (25), (29) of the *Physics Essays* paper. This is a further confirmation that I elaborated using biquaternions, and only biquaternions.

Note also that in line 24 I again used an LHBT to generalize the usual action A, and in this manner I arrived at (2.4), the generalized Schrödinger's equation that was necessary.

5) Now, if we return to consider the previously quoted papers of Santilli on the neutron, one finds that he also derives a generalization of Schrödinger's equation. In the first paper it is contained from page 113 to page 120. In the same manner the other formulation regarding his equation is contained in his second paper from page 196 to (2.8 c) of page 197. Any reader is advised to inform me if he finds some element of derivation and also of formulation that may be considered to be in common. The formal derivations are different, and, in particular, ***the two equations are different.*** Let us consider that in (3.9) of Santilli's first paper there appears b^{-2} and in the second Santilli paper [(2.8b), (2.8c) and (2.10)] there appears ρ, which are not present in our derivation.

So I come to one of the important points of my discussion. My papers go on about the possibility of generalizing Schrödinger's equation. Starting with (2.1) pertaining strictly BQM, I obtained such a generalization which was expressed in (2.4). This equation is profoundly different from the equations that were obtained by Santilli, expressed in (3.6), (3.9), (3.11a), and (3.11b) of his first paper, and in (2.8b), (2.8c), and (2.10) of his second paper.

Note that the difference between the two formulations is characterized by nothing less than a mathematical function that enters in Santilli formulation and not in mine.

This function is introduced as an example in (3.5a) of his first paper. It is $b_k = b_k(t, p, \hat{p}, \psi, \psi^{+}, \ldots)$ and it is called "the characteristic quantities of the medium considered" on line 13 of page 118. As I see it:

- It seems to depend on time t, on p,. . . on ψ, and so on.
- This is a mathematical function that, in addition, is present in a differential equation that still needs to be solved.
- In particular, it is also unknown, as I understand it, in its explicit mathematical expression.

In my formulation this function does not appear. So, the two formulations differ by a fundamental function b_k that appears also in the expression for the binding energy (see (3.16) and (3.19) in Santilli's paper, p. 121) and in other derivations that are of basic importance for the problem under consideration. ***Our derivation, based on BQM, does not give an expression for the binding energy for the system (p, e^{-}) depending on $b_k = b_k(t, p, \hat{p}, \psi, \psi^{+}, \ldots)$.***

Now, I am convinced that if an author proposes a new theory and, in particular, new theoretical elaborations, he always believes firmly in the ideas that he is supporting, and every one is beholden to respect such ideas. Of course, any other researcher, considering such new proposals, must be free to accept or not to accept these results. This is precisely the condition emerging in this case.

The BQM, as it is in my formulation, does not foresee the advent of the basic funcion b_k in the generalization of Schrödinger's equation and in the expression of the binding energy that is consequently obtained when one solves the differential equation (2.4), in *Infinite Energy,* as suggested by Flugge[13] in *Practical Quantum Mechanics*.

As a consequence, the BQM and Santilli's theory are two different formulations that are profoundly different. I repeat: It behooves me to respect any new proposal in science, but I must be free to express my doubts. I am not convinced on the presence of b_k (t, . . .) in the generalized Schrödinger's equation. Considering also the presence of this function in the differential equation, the problem consequently arises of solving this differential equation with the presence of this new function that, I repeat, depends (as well as I am able to understand it) on the time t, and also on the ψ itself. I see that in this manner one goes away from the possibility of obtaining a final solution giving the formation of the neutron.

Obviously, as I have said, any researcher has his motivated ideas when he proposes his new formulation. In fact, starting with page 118 of his paper and continuing on page 119, Santilli states:

> . . . However, if only the global features are needed, such as the average value of the speed of light where passing through our entire atmosphere, then the local index of refraction can be averaged into a constant value n = <n>. Exactly the same situation occurs for the characteristic b-functions which can be effectively averaged into constants for the global behaviour, multiplied by a common isotropic factor $T = T°$ diag.$(b_1^2, b_2^2, b_3^2) > 0$; $b_k = < b_k > =$ const.> 0, k =1, 2, 3.

The reader is invited, obviously, to deepen the argument in the preprint book by Santilli.

In this case, I do not agree with selecting $b_k = < b_k >$ in a so unconstrained manner in quantum mechanics. It is possible that I am wrong, but the introduction of the operation $b_k = <b_k>$ in the course of a theoretical elaboration leads to nothing less than a generalization of Schrödinger's equation [and thus all this in the interest of a theory (the standard quantum mechanics) that is full of mines and where it is sufficient, in light of its modification, to go knock on the doors of other new and unexpected fields]. I repeat, in these conditions I have my strong doubts.

This is the conclusion of the present discussion: ***BQM and Santilli's theory (Hadronic Mechanics, according to the author) are profoundly different, and, in particular, they are also in profound disagreement.***

We may go on confirming such disagreement, continuing to comment on, step by step and formula after formula, my article in *Infinite Energy*. Equation (2.6) is a standard Schrödinger's equation, and so it does not require any further consideration. After, we still have (2.7), (2.8), (2.9), (2.10), (2.11), and (2.12). The elaboration is obtained considering two basic formulas, the $S^{-1} = 1 - |u><u|$ used also by Santilli in his formulations. I could be wrong, but as far as I know, this theoretical expression is due to N. Gisin, whom I quoted in Reference 4 in *Infinite Energy*. Also, preliminary treatments recalling $1 - |u><u|$ and u/ψ used in my elaboration may be found in the papers by Philips and Kleinman.[12]

Let us consider again my paper "A Generalization of Schrödinger's Equation Using Biquaternions . . ." that appeared in *Physics Essays* in 1995, and consider the formulas from (73) to (81). Here, I consider that the Gisin model may be reduced to the study of the non-linear Schrödinger's equation given in (73), and, as any reader may verify, I arrive at (76), to (78), to (79), and (80) that are just the Generalized Schrödinger's equation that I used in (2.4) of my article in *Infinite Energy,* with the addition of the operator $1 - |u><u|$ previously indicated. Thus, in this case, I continued using Gisin's operator expression and not any other. N. Gisin,[14] of course, has published a number of articles on this subject using always the kind of equation given in (74) of *Physics Essays*. We could give a list of such papers. Thus, also this other formula, employed in *Infinite Energy* by me, has been cleared (I hope) for its provenance.

I could also make clear the reason for which I used a derivation of the Gisin expression, but I omit it here for brevity. Only I indicate that in my previous paper in *Physics Essays*, I frequently spoke about possible fluctuations of the quantum of action ħ in some quantum systems, and such fluctuations should be related to variations $\Delta E = (<E> - E)$ of the energy. Owing to fluctuations of the quantum action, here we have something analogous to the intrinsic instability of some well-known quantum systems. As is well-known, such systems are characterized by the energy which, far from being sharp, will be spread over a nonvanishing interval. Here, the reason to select a particular form of $S^{-1} = 1 - |u\rangle\langle u|$ in my formulation.

Thus, in conclusion, also in this case the BQM delineates itself as a theory that cannot be shown to be a derivation of previous elaborations, and, in particular, it does not derive from Hadronic Mechanics. Let us now discuss the presence of the Hulthen potential in equation (2.12) of *Infinite Energy*. Again we must return to my article in *Physics Essays*. This time we must read from eleven lines after (65) to eight lines after (72) where just the Hulthen Potential was calculated.

Here we used the results that were obtained by R.E. Mickens[15] in 1979 and published in *Foundations of Physics*. Anyone may read formulas (1), (2), (3), (4), and (4a) of this paper, and he may establish that, on the basis of these formulas, I introduced (66), (67), (68), (69), (71) and (72) in my *Physics Essays* article.

It is of particular importance to consider (69) where it is formalized explicitly that the potential $V_H(r)$ given in (2.8) and then in (2.12) of *Infinite Energy* is strongly connected to $S^{-1} = 1 - |u\rangle\langle u|$ previously introduced in the same article two lines after (2.6).

This is another basic point that we must consider in detail. In our formulation of BQM, we consider the results established by Mickens in 1979 to be preliminary. On this basis we introduced some formulas in *Physics Essays* and on the basis of these formulas we calculated (in (72) of *Physics Essays*, and we considered in (2.12) of *Infinite Energy*) that $V_H(r)$ is a kind of Hulthen Potential. Thus, also in this case, we have produced our derivation, and it is sufficient to read carefully the article by Mickens to establish the specificity and the particular meaning of our derivation.

Let us consider the important result that we established in *Physics Essays*. Three lines after (69) we explicitly affirm: "In conclusion, we see that the empirical potential (it is $V_H(r)$ considered in *Infinite Energy*) given in (64) is ***essentially dependent*** from V(r) being $T=1-|u\rangle\langle u|$." In other words, we established that in BQM $V_H(r)$ depends on u.

Now, let us consider Santilli's results. In his first paper, he considers the Hulthen Potential in (3.9), (3.10), (3.11a), and (3.11b), and, after (3.9), he affirms: "The derivation of the Hulthen Potential was based on the addition to the Coulomb system (3.1)"(that is the equation on page 116, a standard Schrödinger's equation)" of a variationally non-self-adjoint force. That is, a force violating the integrability conditions for the existence of a Hamiltonian,[3,5] in order to ensure indeed the transition from the exterior Hamiltonian to the interior non-Hamiltonian problem. This derivation will not be reviewed here for brevity." He recalls papers[3,5] that should be:

3) R.M. Santilli. 1978. *Hadronic J.*, **1**, 228
5) R.M. Santilli. 1978. *Foundations of Theoretical Mechanics Vol. I-III*; Springer-Verlag, Heidelberg, NY

I have not read these works of Santilli, but I do know about variationally non-self-adjoint conditions and I exclude that any link may exist between a formulation recalling such conditions and my formulation recalling the results of Mickens.

In his second paper Santilli introduces the Hulthen Potential in (2.10), (2.11), and (2.13). Also in this case we have not ***any direct derivation of this potential***. Two lines after (2.10) it is stated: "Paper[9] has shown that the above equation"(it is that one including the Hulthen potential) "emerges precisely as the isotopic lifting of the conventional radial equation for the Coulomb interaction under assumption (2.8)" (these are the formulas (2.8b) and (2.8c) of his paper) "for the isounit, according to the expressions. . ." (and (2.11) are given). In this case it is recalled in his Reference 9 that in the list of the references it comes from paper "in press." So, also in this case we have not a direct derivation of the Hulthen Potential, and, in any case, it appears strongly evident that the arguments used for BQM to consider $V_H(r)$ are totally different from the reason considered instead in the other theory.

There is more. It emerges again and in an evident manner the profound disagreement existing between BQM and the Hadronic Mechanics of Santilli. Let us consider what he affirms in his first paper just after considering the Hulthen Potential in (3.11a) and (3.11b) on page 120. He affirms:

> ***Charge independence of the cold fusion of particles:*** the central mechanism of the expected the cold fusion of particles is that the deep mutual penetration of the wave packets at mutual distances equal or smaller than 1 fm implies ***the dominance of variationally*** non self adjoint, non linear non local-non hamiltonian forces representable via an isotopy of quantum mechanics, ***which are attractive irrespective of the attractive or repulsive character of the original Coulomb interactions at large mutual distances.***

The same concept recurs also in page 104, lines 32 and 33. Let us remember instead the final statement reached previously by BQM. According to this statement ***the expression of $V_H(r)$ is strongly linked to u(r), which is connected to the particular expression employed for V(r). In our derivation V(r) determines u(r) according to the solution of Schrödinger's equation and the analytical form of u (r) strongly determines the analytical, mathematical, and physical behaviour of $V_H(r)$. If we consider $V(r)=-e^2/r$, we have a given u(r) and thus a given $V_H(r)$. If we consider $V(r)=+e^2/r$, we obtain a totally different expression for u(r) and thus a different expression for $V_H(r)$.*** This is in BQM. According to Hadronic mechanics, it is, as previously stated, "irrespective of the attractive or repulsive character of the original Coulomb interactions at large mutual distances." ***It is so evident, one more time, that the two theories go in two different directions, they speak totally different languages, they predict totally different results, and they are in profound disagreement.*** I remember here, in particular, the central role explained by $V_H(r)$ in these formulations. Thus, we have dissonance at the foundations of the two formulations. I give a detailed example in this regard.

In the comments regarding my papers on *Infinite Energy* it was cunningly avoided to consider that it also contains page 54 where there is reported a detailed calculation on the (d, d) reaction and the modification of the barrier as we may obtain it by using BQM. This is just a case in which we have a repulsive Coulomb potential and we have the problem of calculating the overlap of wave functions in BQM. We are at so different a plane of theoretical conceptualization of a theory, with respect to Hadronic Mechanics, that we are induced to consider in our case the presence of electrons, and, with respect to this particular situation, we calculate $V_H(r)$. Let us examine the text of page 54 and formulas (6.1), (6.2), (6.3), (6.4), (6.5), and (6.6) to confirm that BQM is in profound disagreement with respect to Hadronic Mechanics. Also, let us examine the two lines previous to (6.7). It is stated that "if one performs calculations for the potential based on the above uniform electron-density model (that is, if one performs calculations by classical electrodynamics) one finds a (6.7) that is in perfect accord with (6.6)." Thus, BQM is in agreement with standard physics and quantum physics when it considers $V_H(r)$ and the overlap of the wave functions. Consequently, let us read Hadronic Mechanics to see again the basic differences and the profound disagreement between the two formulations. There is still more.

I have mentioned my paper in *Infinite Energy* arriving at

(2.12). Go to page 52, and find formulas from (4.1) to (4.7). There is some point that still needs attention. Equation (4.6) gives the expression for the binding energy in our BQM formulation, and, I have discussed previously that we have some important differences with respect to Santilli's papers. Let us examine (4.5) with the physical conditions expressed after two lines. We have that it must be

$$2mV_0' > \hbar^2 b^2$$

with $V_0' = e^2 b + V_0$ and $V_0 = \alpha_s E_\mu$. Solving the previous equation, one finds the range of values that b may assume. One finds that $0 \leq b \leq 5\text{x}10^8$ cm^{-1}. This is the evaluation of the BQM. Now, let us go to the papers of Santilli.

As I read the paper, the problem is explained in formulas (3.9), (3.10), (3.11a), (3.11b), (3.12a), (3.12b), (3.12c), (3.12d) on pages 119 and 120, and from (3.13) to (3.24) on pages 121 and 122. In particular, we must give attention to (3.9) and (3.10), and to (3.23). Let us observe one of the several ways in which the Hulthen Potential is written. It is

$$\frac{V_0 \exp(-R^0 r)}{1-\exp(-R^0 r)}.$$

In (3.12d) R^0 is defined as the "charge radius (cm)," and in (3.23) it is given $R^0 = 0.80\text{x}10^{13}$ cm.

So, I cannot understand the formulation of this paper. How can R^0r be adimensional? Can I attempt to understand that it should be r/R^0 instead of R^0r? In this possible case we should have a b-parameter in the Hulthen potential

$$b = 1/R^0 = 0.125 \text{ x } 10^{14} \text{ cm}^{-1}.$$

If this is the correct interpretation, the difference between the calculated value of b of BQM and the calculated value of Santilli differ by about six orders of magnitude ($\approx 10^8$ cm^{-1} with respect to $0.125\text{x}10^{14}$ cm^{-1}).

We have an additional confirmation that we are speaking of two absolutely and completely different things with BQM and Hadronic Mechanics.

Note that the value $R^0 = 0.80\text{x}10^{13}$ cm is used by Santilli on pages 122, 123, 124 of his paper to conclude positively about the possible formulation of the neutron. It is difficult to understand also how the assumed value of R^0 may agree, soon after, with the possible value of V_0' as it is calculated in BQM, and thus in my paper in *Infinite Energy*. In fact V_0' results given to be

$$V_0' = e\,b + E_u\alpha$$

where E_u is a few eV and the overlap coefficient results in $\alpha_u \leq 1$. ***Thus, still again we have two different formulations that are in profound disagreement.***

All the points of dissonance reflect basic points of the formu-

Relativity by Biquaternions

In 1997 I published a paper in *Physics Essay*[7] entitled "On a Generalization of the Physical Laws by Biquaternions: An Application to the Generalization of Minkowski Space-Time." Let us go to the essential points regarding our discussion. Twenty-six lines before (9), the object of the paper is clearly evident. We intended to construct relativity in a useful form for the previously introduced BQM, and still again, we intended to pursue this objective using only biquaternions, as it is in all our scientific activity. How to use biquaternions in relativity?

The answer is in lines 9 to 24 in the second page. There is the evident motivation that biquaternions have an intrinsic relativistic character (the norm of a biquaternion fully reproduces the basic invariant of relativity). We may thus consider the biquaternion space where a given biquaternion corresponds to the point $P(z_\mu)$ (μ=0, 1, 2, 3) in the complex four dimensional space (z_0, z_1, z_2, z_3) and vice-versa. For real biquaternions, we have the usual Minkowski space-time. This is the scenery of our elaboration: biquaternions, and only biquaternions.

Considering special relativity, what is the first thing that a physicist could perform also using biquaternions? I find it logical to acknowledge that the first step should be to introduce Lorentz transformations. This was actually what happened in my paper from (9) to (16). I derived the Lorentz transformations as clearly expected by any physicist in any previous formulation of this kind.

What was the old/new feature of this nth (my) derivation? The old/new fact was that to obtain the Lorentz transformations, they obviously required those LHBT that we introduced in BQM. We do not repeat here the meaning that we attributed to LHBT in BQM, including also the possible identification of the hidden variable. We do not return on this well-known point that now is reproposed in our study of relativity. Thus, we found the Lorentz transformations, and we found that we may speak of hidden variables also in the case of relativity and such "hidden variables" were indicated in (16).

There is another important point. We found that biquaternions give us the usual relativity (the standard invariant, the Lorentz transformations, and so on) using LHBT with real coefficients. Soon after, it appeared very important to examine what happens when passing from an LHBT with real coefficients to an LHBT given with complex coefficients. This was precisely the effort that we pursued from (17) to (23).

We considered an LHBT made with complex coefficients, see (17), (18), (19), and, according to LHBT, we obtained the transformed biquaternion (18) that is

$$Z = x_0 + x_1e_1 + x_2e_2 + x_3e_3 \xrightarrow{\text{LHBT}} Z' = x_0'e_0' + x_1'e_1' + x_2'e_2' + x_3'e_3'$$

Let us remember the initial statement: To a given biquaternion $Z = \sum_\mu z_\mu e_\mu$ there corresponds a point $P(z_\mu)$ in a complex four-dimensional space (z_0, z_1, z_2, z_3) in which rectangular coordinates are defined by a set of four complex numbers z_μ and vice-versa. A subset of real biquaternions defines a real biquaternion space, and the norm of the real biquaternion is the quadratic metric of Minkowski space. Thus, according to the previous formula, we had a point event $P(x_\mu)$ and by the LHBT a point event $P(x_\mu')$. There have been produced a lot of papers with this framework, and, in particular, K. Imaeda considered this approach in 1975 and also previously starting in 1950.

Let us consider five lines after (20) in our paper in *Physics Essays*. We affirm: "In other words, in order to study a space-time different from the space-time of a vacuum, we introduce a transformation that is able to transform the coordinates (x_0, x_1, x_2, x_3), ***corresponding to an event in vacuum***, into the coordinates (x_0', x_1', x_2', x_3'), corresponding to the event ***in the space-time under consideration.***" This was the aim of our used LHBT.

There is more. We asked, in particular, if we could have that $x_\mu' = b_\mu x_\mu$ (see ten lines after (20)) with b_μ characteristic functions delineating the basic physical features of each space-time taken into consideration. These functions were explicitly calculated in (19) and they contained a functional dependence from the hidden variable (which obviously was introduced by the LHBT). The expression was thus also an expression of a hidden variable contained in the problem. ***In any case, we established a correspondence between*** x_μ ***(***μ***=0, 1, 2, 3) and*** x'_μ ***(***μ***=0, 1, 2, 3).*** After this step, we gave analytical solution to our problem by (20) and (21) and, in this manner, we obtained mathematical and physical confirmation that it is possible to consider $x_\mu' = b_\mu x_\mu$ where, I repeat still one time, x_μ and x'_μ are only the coefficients of the two biquaternions, Z and Z', respectively.

This is the derivation that we performed to obtain (22) and (23) that we introduced after in (3.9) and (3.10) of the paper in *Infinite Energy*. Obviously, owing to the obtained transformation, we were conscious that our established correspondence between biquaternions Z and Z′ now enabled us to consider that x_μ should represent "an event without structure" and x'_μ an event that implies an anisotropical structure owing to the presence of the characterizing functions b_μ .

There is another point of great and fundamental importance. We established that the correspondence between x_μ and x'_μ is realized by b_μ (which also contains hidden variables), ***but we do not touch the relativity in any of its point. In fact, in our formulation the generalization of the invariant appears only when we consider the event P(x′μ) with respect to the frame that contains P(xμ). Only in this case we have (22) and (23) from Physics Essays, and (3.9) and (3.10) from Infinite Energy. When, instead, we consider P(x′μ) in its frame, we have obviously that*** $Z'=x_0'+x_1'e_1'+x_2'e_2'+x_3'e_3'$ ***that preserves the usual norm of***

$$Z', N(Z') = x_0^2 - x_1'^2 - x_2'^2 - x_3'^2$$

and it preserves also the usual quadratic metric of Minkowski space. Thus, the kind of generalization that we operated by biquaternions is in perfect accord with the standard theory of relativity and in accord with our general BQM.

There is still another point that we must examine. Owing to $x_{\overline{\mu}}= b_{\mu}x_{\mu}$, when b_μ=constant/x_μ , we have possible cases in which a given particle is moving in $P(x_\mu)$ with coordinates that vary in time (according to $x_\mu(t)$) while it is arrested in $P(x'_\mu)$. It was this important feature that enabled us to consider that this particle may have two different rest-mass values in the two space-times respectively, and it was for this reason that we renormalized the mass of this particle as explicitly indicated three lines after (4.7) in my paper in *Infinite Energy* and in (26), (27), (28) of my paper in *Physics Essays*. When the electron enters into the proton, it must be at rest in this medium. It is instead in motion with respect to its observation in the external medium of the proton.

I must now conclude: I have considered all the formulas of my paper in *Infinite Energy* from (1.1) to (2.12), from (3.1) to (3.9), and from (4.1) to (4.7). For each formula I have indicated its specific derivation in the picture of the algebra of biquaternions, and of the BQM.

The reader is now invited to read the number of papers that Santilli has produced on the possible generalization of the invariant in special relativity with particular regards to the papers that I have read and that appeared in the preprint book, *Hadronic Mechanics*. Santilli considers events (x_0, x_1, x_2, x_3) in space-times whose metric may be

$$x_0^2 - x_1^2 - x_2^2 - x_3^2 \qquad (x_0x_0 - x_1x_1 - x_2x_2 - x_3x_3)$$

as it is usual in the standard theory of relativity, and the metric

$$x_0b_0^2x_0 - x_1b_1^2x_1 - x_2b_2^2x_2 - x_3b_3^2x_3$$

in modified, generalized, and anisotropical space-times. Characteristic functions of these considered space-times—b_μ(μ =0, 1, 2, 3)—do not contain hidden variables. As I pointed out, Santilli's formulation ***does not consider functionals*** $x'_\mu = b_\mu x_\mu$ ***that instead are of basic importance in our derivation and this shows the basic differences of mine with respect to Santilli's formulation.*** In addition, we remain in the standard relativity with our generalization of the standard invariant. Thus, apart from the formal analogy between the previous invariant and that one obtained by me in BQM, it may be seen that, still again, the reason for conceptual differences and disagreements between the two formulations seem to be more than the reasons for an agreement.

In any case I have derived all my formulations still using biquaternions and LHBT. Still, consider that b=b(. . .) in my papers depends an a=a(. . .), b=b(. . .) that define the employed LHBT with a basic difference with respect to Santilli. There still remains the question that I used the experimental value of b_0 (given by Santilli *et al*. in Reference 9 of *Infinite Energy*) in my paper in *Infinite Energy*. I used this value tentatively as it is established in my paper in *Infinite Energy* where, in fact, I wrote "according to the possible" (but not established) "value b_0 = 1.65 given in Reference 9." Let us note that the value of b_0 was tentatively considered by us as the only given experimental datum. It is not indigenous to our BQM treatment. The reason is that the dynamics of neutron formation happen with some substantial differences between BQM and Hadronic Mechanics as we are considering now in detail. Also inserted was a comment on the calculated value of the binding energy.

On the basis of the assumed value of b_0, the corresponding value of 0.072 MeV for the binding energy was indirectly obtained to give the mass of the neutron. In fact, we wrote:

$$(p, e^-) \text{ system: } (938.279 + 1.309 - 0.072) \text{ MeV} = 939.153 \text{ MeV}$$

which is the same as writing

$$(938.279 + 1.309 - x) \text{ MeV} = 939.153 \text{ MeV with } x= ?$$

In any case, we have established our comments and our points of basic differences between the two formulations, and also, in the case of the possible generalization, of the relativistic invariant of special relativity. It remains now to comment on the paragraph regarding the spin.

On the Language Used in the Paper in *Infinite Energy*

In the paragraph entitled "A Brief Note on Biquaternions," I use only the language concerning biquaternion algebra, and so I cannot see contact points with Santilli's elaboration.

In the paragraph entitled "The Basic Physical Features of the BQM," I use only an application of the language of biquaternions to the problem, and so, also in this case, I do not see the contact points with Santilli's formulation. An exception is represented six lines after (2.7) where we use the following terms: "pseudo-potential $V_H(r)$," and "integral of wave overlapping." In detail, also nine lines after (2.9), I use the following terms: "In addition to the conventional, local, action at-distance, potential" we have to add ". . .other interactions that are non-linear, non-local, and non-potential interactions, and they are due to the wave-overlapping." Two lines after (2.10), I affirm: "The central mechanism expected is the deep mutual penetration $< u / \psi > \neq 0$ of the wave packets." In these phrases the following terms are involved: pseudo-potential $V_H(r)$; integral of wave overlapping; local-action at-distance; non-linear, non-local and non-potential interactions; wave-overlapping; deep mutual penetration $< u / \psi > \neq 0$.

Two concepts are involved: 1) wave-overlapping as it results from (2.9) in *Infinite Energy* with non-local, non-linear, and non-potential interaction, and 2) local-action at-distance that regards, instead, the usual potential V(r) considered in (2.6) in *Infinite Energy*.

I ask: what should any physicist call the quantity $\alpha_s =< u / \psi>$ resulting in (2.9)? Does any physicist know some term that is different from wave-overlapping to describe (2.9)? Look in any textbook of quantum mechanics (see for example P.T. Mathews,[16] pages 25, 26, 33, 82, 134, 137, and 141), and you will verify that the concept of wave-overlapping belongs to the primary foundations of wave mechanics and, later, of quantum mechanics. Examine the book of D. Bohm[17] from page 583 to page 629, and you will recognize the general importance of this concept in all quantum mechanics.

The concept of wave-overlapping belongs thus to the foundations of quantum mechanics and to its standard terminology, and so it should be used freely by any physicists using QM. Concerning the concept of wave-overlapping with regard to an interaction, we are in the same condition. The concept of wave-overlapping as an interaction may be found, for example, in the fine book of A.V. Davydov[18](see, for example, the pages from 14 to 17, where Davydov explains some foundations regarding the quantum aspects of the chemical bonds); the same is also in any book of quantum chemistry. Let us now also consider nuclear physics, and, in particular, heavy ion transfer reactions. Here is the overlap integral of the wave functions which represents an interaction and determines the neutron transfer between two nuclear cores (see, for example, any textbook on stripping reactions or K.R. Grider[19] or W. Tobocman[18]). It is within this framework that we use the concept of wave-overlapping, of wave-overlapping interaction, of the pseudo-potential nature of wave-overlapping (see, in particular, the previously indicated papers to confirm that the wave-overlapping interaction is not derived from a usual, action at-a-distance potential and must be considered as a pseudo-potential). The terms "non-local" and "non-linear" derive directly from the explicit mathematical expression $\alpha_s = <u/\psi>$ that, as it is well-known, involves the explicit calculation of an integral.

Thus, we used standard terminology that belongs to the standard foundations of QM, and the meaning that we attributed to these used terms must be considered to be in line with these standard foundations of the theory. Let us explain in more detail.

The development of the generalized Schrödinger's equation by biquaternions, (2.4) in *Infinite Energy*, leads to the expression for $V_H(r)$ that is given in (2.8). Let us remember that, in order to give an explicit expression to (2.8) in *Infinite Energy*, we followed in *Physics Essays* the paper of R.E. Mickens who considered that the range R of any interaction must depend only on $\hbar$, c (the speed of light in vacuum) and the energy of interaction. Starting with this basic point (see (1) and (2) in Mickens' paper), we considered a possible fluctuation of the quantum action inducing $V_H(r)$ to be pseudo-potential having the explicit expression of a Hulthen Potential. When I calculate explicitly the quantity

$$< u / \psi > \frac{u}{\psi}$$

I find that the actual calculation of this expression involves a consideration of the region of the space in which it happens so that

$$\frac{\exp(-\alpha r)}{r} \cong b .$$

This is explicitly indicated in page 54 of my paper in *Infinite Energy* (the first line after (6.2)). In this case, and only in this case, I have a confirmation of the result obtained following Mickens. In other words, only in the range $\exp(-\alpha r) \cong br$ is it certainly true that we may express the previous $< u / \psi > u / \psi$ by the analytical expression of the Hulthen Potential. In detail, only in this range do we have accord between the result that one obtains following Mickens (when admitting a possible fluctuation of the quantum of action $\hbar$) and the possible direct calculation of $<u / \psi> u / \psi$. Thus my derivation of $V_H(r)$ strongly depends on $b = \exp(-\alpha r)/r$. It is this value of b that characterizes my derivation, and it gives meaning to the terms wave-overlapping, wave-overlapping interaction, and non-linear, non-local interaction with respect to the usual action on at-distance: these are, of course, all the terms that I used in *Infinite Energy*.

Now, it is certainly true that Santilli introduced terms such as wave-overlapping, wave-overlapping interaction, pseudo-potential interaction, non-local, non-linear, non-potential interactions with respect to the usual-action-at-distance interaction, and so regarding all the terms under consideration. It is sufficient to read his preprint book *Hadronic Mechanics* (repeatedly quoted here) to verify that he has devoted his complete studies to this subject. However it is evident, at the same time, that behind the apparent similarity of the used terminology, we are speaking about two things that, conceptually, have profound differences. Here the question of the range *1/b* is essential to support the wave-overlapping model and the wave-overlapping interaction model about which we are speaking. Let us remember that, as was previously shown in detail, I go with a range $R = 1/b \cong 10^{-9}$ cm and Santilli calculates (if I understand his exposition in Vol. III of the preprint book of *Hadronic Mechanics*) $R \cong 0.8 \times 10^{-13}$ cm. ***This is an essential point that marks the basic differences when I speak of wave-overlapping and wave-overlapping interaction with respect to Santilli's conceptual foundations of Hadronic Mechanics.*** Let us examine in detail the foundations of Hadronic Mechanics as characterized in Vol. I, II, III of his book.[11] Let us consider in detail the conceptual statements of his formulation, frequently repeated in Volume I (chapters 1 and 7), Volume II (chapters 1, 2, 11), and Volume III (chapters 7, 9, 10, 11) of his book, and any one can verify the profound differences existing between BQM and Hadronic Mechanics, regarding overlap and all the other statements.

Thus, just as I wrote in previous letters to Infinite Energy, the differences between BQM and Hadronic Mechanics are at the very foundations of the two theories. So, I hope I have also cleared up this point.

Let us examine now the paragraph entitled, "On a Possible Generalization . . ." of my article in *Infinite Energy*. I think that the point that could be questioned regards the lines from 31 ("The validity of the usual relativistic invariant. . .") to 42 where I say that the validity of electromagnetic interactions, of the standard relativistic invariant and of point-like particles moving in empty space under electromagnetic interactions, is strongly established. The other point is "the generalization of (3.10) states that in some cases the particles cannot be approximated as massive points."

Santilli has considered these two problems in his book and, thus, previous to me. He has explained these concepts previous to me, and he has extensively studied this subject particularly with respect to me. So in comparison to Santilli, I cannot be considered to have a particular competence in relativity. However, my general knowledge in theoretical physics (even if not particularly devoted to relativity) enables me also to know that "the validity of the usual relativistic invariant for electromagnetic interactions is strongly established by a considerable number of experimental confirmations." This is so general a statement and such universal knowledge in physics that I cannot think that it is not known to a student of physics. So it is also for isotropy (anisotropy) of space-time and other similar foundations. Also, the point that particles cannot be approximated as massive points in some cases re-enters in the standard picture and is common knowledges in physics. So this enables me also to use these conceptual foundations.

On the other hand, I repeat, Santilli has dedicated several studies to these problems that he has explored from different viewpoints, and BQM has not had, and it will not have, direct interest in this regard. Never was my aim to apply BQM to these themes: using biquaternions, I used an LHBT with complex coefficients applied to biquaternions (whose interpretation as events in space-time was clarified by me previously) and this was done only to have useful formulas for BQM. The impossibility of considering, in some cases, the particles as massive points was deduced from the formula $x'_\mu = b_\mu x_\mu$ that was previously discussed and where it is the presence of b_μ that guarantees that these are the cases in which the particles cannot be considered as being massive points. In any case, reading any book on physics, it is easy to find the concept of a particle having structure.

On the other hand, in my paper in *Physics Essays*, "On the Generalization of the Physical Laws. . .," I discussed this point regarding particles considered as massive points with respect to the case in which this situation does not occur. Again, I considered this problem using (as always in all my formulations) biquaternions. Let us examine the conclusion of this paper from line 37 to line 113. I outlined there the central role explained by LHBT in biquaternions to obtain BQM. I outlined that biquaternions have an intrinsic relativistic character (and, in fact, we discussed previously their strong connection to special relativity, Minkowski space-time and the use of LHBT to have Lorentz transformations). Then in the following is obtained another important feature: ***in these lines I show that Maxwell's equations in vacuum are promptly derived by using biquaternions*** (see in particular (30) and (31) but various kinds of derivations also exist in the literature). It is in this way that I came to the conclusion ***that biquaternions relate the electromagnetic interactions (when employed in their standard form). Biquaternions represent simultaneously the electromagnetic fields and the basic objects that they relate, biquaternions also represent the basic feature of the space-time for the vacuum (and, in fact, we have seen their relativistic character). Thus the conclusion of these results is that we have a unification of the usual Minkowski space-time, of the laws of physics (Maxwell's equations*** $\Leftrightarrow$ ***electromagnetic interactions) and objects (electromagnetic fields) into a simple entity and this entity is represented by biquaternions. The sets of axioms of Minkowski space-time, of Maxwell's equations, of electromagnetic fields were erroneously considered in the past to be mutually independent: they are manifestations of a single entity based on the set of axioms of biquaternions.***

This was the conclusion that I considered to be of great importance and that I derived in the paper in *Physics Essays*. This result was added to the generalization obtained by an LHBT ($x'_{\mu} = b_{\mu} x_{\mu}$) and they enabled me to express both of the two phrases reported in *Infinite Energy*, relating the usual relativistic invariant, the electromagnetic interactions and the point-like particles on the one hand, and the generalized invariant with $x'_{\mu} = b_{\mu} x_{\mu}$, other interactions and particles not as massive points on the other. I considered a strong indication that Minkowski space-time, electromagnetic interactions, and physical objects only may be derived in a unified manner from biquaternions.

Finally, we also had a comment concerning the lecture of the book of E. Fermi as stated on page 51 of my article in *Infinite Energy*. I confirm that this book also arrived in Italy, and enabled me to read it, as well as the articles of P.A.M. Dirac, as explicitly indicated in Reference 10 in *Infinite Energy*.

This statement concludes my discussion of the language used in my paper in *Infinite Energy*. I have examined line by line the terms used, including also the ***conclusions*** of my paper in *Infinite Energy* where the terminology previously discussed was also used.

It still remains to discuss the problem of the spin of the neutron. The theory of Dirac uses biquaternions as, of course, I showed previously and in all my previous papers. Again, here we use biquaternions to represent the spin of the particle. We use biquaternions at order n=4 since BQM does not use eigen functions of operators (as, of course, we stated previously): when considering a system of two particles, we must consider two different sets of operators E_{0i} (i=1, 2, 3) and E_{i0} (i=1, 2, 3) to represent the spins of the two particles and to assure their simultaneous measurability. For this reason commutative biquaternion basic elements ($E_{0i} E_{i0} = E_{i0} E_{0i}$ $\forall$ i) must be considered. Again by using LHBT we may transform E_{0i} in $E_{0i}(\lambda)$ depending, as usual in BQM, upon the hidden variable $\lambda = \lambda(. . .)$. This hidden variable is selected afterwards to give a total angular momentum equal to zero for the particle. This is my procedure in BQM. Anyone may now read the Santilli papers. In his first paper he solves the problem from page 125 to 132, and in his second paper he examines the problem from page 201 to 204 in a completely different manner. ***Again, in this case we are on two different planes of formulation.***

It remains to discuss briefly the problem of the invariance of BQM. Santilli writes in his paper in *Infinite Energy* (**5**, 25, p. 66, 1999): "The reader should be made aware up front that all publications on hadronic mechanics (including mine) prior to 1996 are physically inconsistent due to lack of full invariance, which was recently achieved in. . ." (see the text for details). Here it is Santilli speaking about this theory. This is another reason to consider that BQM has profound differences with respect to Santilli's theory.

I reiterate:

1) I use only biquaternions and LHBT.

2) I show that all the standard quantum mechanics are a biquaternion calculus. In particular, I show that in the standard QM we have only the basic operators e_i (i=1, 2, 3) that are the basic elements of the biquaternions considered at various orders n=2, 3, 4. . .

3) In my paper in *Physics Essays* I show that any other quantum observable or any quantum operator of the usual quantum mechanics is realized by the e_i (i=1, 2, 3) and only by e_i (i=1, 2, 3; n=2, 3, 4. . .). Let us recognize fully the importance of this result. It enables us to confirm that quantum mechanics is a biquaternion calculus (for details, see the paragraph entitled "On the Rules of Biquaternion Quantum Mechanics" in *Physics Essays* (one may see, in a simplified form, Jordan[8] particularly the chapter entitled "All Quantities made from Spin").

4) I repeat: these are the basic points of the BQM. All the quantities are made from the basic elements e_i of biquaternions (i=1, 2, 3; n=2, 3, 4. . .). I am here to see where it is that some other theoretical formulation, according to BQM, is arranged about the same statements as 1, 2, 3.

5) These are the statements and from these statements the generalization may start. Using LHBT we have that

$$e_i'(\lambda) = U(\lambda)\, e_i\, U^{+}(\lambda).$$

In other words, using LHBT, the basic elements of biquaternions may be generalized. Since in (1,2,3) ***we have said that all the quantities of QM are made by*** e_i ***(i=1, 2, 3; n=2, 3, 4.....), it follows that now, having the generalization*** $e_i \rightarrow e_i(\lambda)$***, we have consequently a generalization of all the quantities of quantum mechanics, and, in detail, of all its operators, basic commutators and so on*** as we employed them in my paper in *Infinite Energy*. Again, I want to see where it is that some other theoretical formulation, according to the BQM, is arranged about the same statements (1, 2, 3, 4, 5). I repeat, all this is expressed in the basic rules of BQM in my paper in *Physics Essays* and in Jordan[8] where LHBT transformations are used, for example, in chapters 21 to 28. So, in conclusion, anyone reading BQM must fix in his mind the points (1, 2, 3, 4, 5) and soon after, on this basis, he may explain where it is that BQM may be considered with regard to other formulations.

6) Let us consider the biquaternion $Z = x_0 + x_1 e_1 + x_2 e_2 + x_3 e_3$. If we use an LHBT, we have $Z' = x_0\, UU^{+} + x_1\, Ue_1 U^{+} + x_2\, Ue_2 U^{+} + x_3\, Ue_3\, U$. This is to say that e_i are changed in $e_i'(\lambda)$ and the new unity of such field of biquaternions now ***becomes*** not 1 but UU^{+}. We have new basic elements of biquaternions (UU^{+}, e_1', e_2', e_3') and here we do not have a lack of invariance, as anyone may verify.

Comments

I have now completed my exposition about my paper that appeared in *Infinite Energy* and everyone may verifiy that I have examined in detail every formula and, possibly, every word. The result has been that I have shown that all points, problems, and formulas of my paper were elaborated exclusively within the framework of BQM, using only biquaternions and LHBT and using derivations and formulations that proceed independently from the inner formalism of the biquaternion algebra that was employed. And this was, of course, the primary objective for writing the paper: precisely to show the ability of BQM to treat in detail the problem of the possible formation of the neutron using only the simple instrument of biquaternions and BQM. As I stated in a previous letter in *Infinite Energy*, Santilli has priority to all of his ideas and all his results concerning Hadronic Mechanics and concerning, in particular, the study on the formation of the neutron. All of these ideas, all of these results were introduced previously to BQM, and I never aimed to disclaim his paternity on all these results. I only aimed to show that BQM may treat in an independent way (that is, by using biquaternions, LHBT, and the exposed conceptual foundations of BQM) the problem of the neutron. As everyone may have verified, some basic conceptual and formal differences arise between the two formulations, and, in my opinion, they identify the reasons for profound disagreement between them. In any case, they confirm that we have two totally independent elaborations even if some formal and terminological contact points should induce some inattentive reader to believe to the contrary.

There is a particular point on which I still insist. It involves the problem of the binding energy regarding the formation of the neutron. I confirm that tentatively I assumed the value of b_0 as indicated by Santilli (Reference 9 in *Infinite Energy*), but it was only a value for the calculation. While in Hadronic Mechanics the problem is solved on the basis of the results that appeared in the preprint book, *Hadronic Mechanics* by Santilli, in BQM the problem is not considered solved. The actual nature of the process when the electron penetrates inside the proton must be defined. As it may be shown,[20] in fact, BQM is in perfect accord with quark theories. BQM, in particular, introduces some biquaternion variables showing the perfect reception of the quarks as existing physical objects. It is in light of this important result that we consider the penetration of the electron inside the proton and we aim to solve definitively the problem of the formation of the neutron in this direction. As I understand Hadronic Mechanics, this is not exactly the case for this theory. Thus, again, BQM and Hadronic Mechanics, in my opinion, have a profound disagreement between them. On the other hand, the tentative idea of considering the presence of leptonic matter in the neutron goes back several years. There are, for example, some models that appeared in 1956 and 1960 by Z. Maki, M. Wakagawa, Y. Ohnuki, and S. Sakata on *Progress of Theoretical Physics* (**16**, 686, 1956 and **23**, 1174, 1960) in which the presence of the electron inside the neutron was conceived and discussed in detail. Papers by J. Strathdee, Novack, Woo, Sucher, and Salam continue to this day. The neutron was carefully studied along this line, stimulated possibly by the paper of Rutherford. BQM moves in this direction.

I must add a final comment. Notwithstanding the profound differences between BQM and Hadronic Mechanics, I always quoted Santilli for his results.

Let us consider my papers in *Infinite Energy*. There appeared an experimental paper in Volume 4, Issue 23, 1999 (and I will call it [1]). Two papers (an experimental one, [2]; and a theoretical one [3]) appeared in Volume 4, Issue 24, 1999.

I wrote these papers simultaneously, and, in particular, papers [2] and [3] appeared in the same journal since they were written simultaneously as a single text. I wrote a single article, divided it into two parts, and gave two subtitles to differentiate the theoretical from the experimental exposition. It was also a matter of off-print that the editor of *Infinite Energy* edited [3] before [2] and not [2] previous to [3]. It was only a trivial matter. In any case I wrote only one article, 2+3 or 3+2, and I quoted on the basis of this unique text.

Now, let us consider paper [2]. At the first line of this article (I repeat, ***at the first line***) I say the first two words of all the paper: it is "Rather recently (1)" and immediately I quote Reference 1. In Reference 1, I quote some of my papers on BQM to indicate to the reader where he can find the articles to actually understand BQM and soon after I say: "For a ***detailed*** exposition . . ., and ***for an historical account of previous works regarding the possibility of formation of the neutron by proton and electron***, see my paper [3] in Infinite Energy at the References 3, 4, 5, 6." Santilli is in ***6***. Since the two papers [2] and [3] were written and published simultaneously, it was clearly indicated that Santilli wrote "***previous works regarding the possibility of formation of the neutron by proton and electron.***"

Note that I quoted after two words at the start of my paper (I repeat, two words, not after two centuries of time, only after two words); I recalled these references [3, 4, 5, 6] (I quoted Santilli in [6]) soon after deriving (2.4) which is the basic equation of the whole paper. I wrote, speaking about the fundamental equation (2.4): "***some authors*** [3, 4, 5, 6] (and 6 is Santilli) ***identified equations in agreement with equations*** (2.2) and (2.4) (in *Infinite Energy*) ***and thus with the elaboration of the present paper.***" I repeat "***in agreement with equations*** (2.2), (2.4) and ***with the elaboration of the present paper.***" What should it mean that previous works [3, 4, 5, 6] ***are in agreement*** with my equations (2.2) and (2.4) and ***are in agreement*** with the ***elaboration of the present paper?*** This phrase indicates clearly that previously in [3, 4, 5, 6] equations were obtained in agreement with mine and that these authors discussed, previously, the problem of the formation of the neutron by p and e^- as in my present paper. It is even more clear if someone considers that these phrases comment on the previous statement: "***previous works regarding the possibility of formation of the neutron by proton and electron***" reported in the first line of my paper [2]. Don't these two phrases clearly express that Santilli obtained previously, with respect to me, an equation in agreement with (2.4) and that he studied the problem of the formation of the neutron previously, with respect to me? Only a person that is unable to understand objectively the facts could think that I did not quote Santilli and that I did not send the reader to read all his results obtained previously, with respect to me. Note that I quoted him in spite of the basic differences between BQM and Hadronic Mechanics as previously discussed.

Let us explain how it was that I quoted simultaneously six different authors in [3, 4, 5, 6]. T. Phipps was quoted since he produced in 1960 an equation having points of contact with (2.4) in *Infinite Energy*, and he also was interested (if I do not misremember) in the possible presence of leptonic matter in hadrons. This happened in 1960 and his generalized equation is in line with (2.4). N. Gisin produced various papers and, in my opinion, he introduced the necessary form for $S^{-1} = 1 - |u\rangle\langle u|$ used in his and in my generalized Schrödinger's equation. M. Kostin and E. Kamai also produced equations that, in my opinion, are in accord with (2.4) and so I quoted these authors for at least one of the two basic reason [***accord of the equation, previous studies*** on the (p, e^-) formation of n]. Again P. Caldirola produced an equation that, in my opinion, is similar with (2.4) in *Infinite Energy*. In addition, P. Caldirola was also interested in the problem of the formation of n by (p, e^-), and in particular, he was also interested in the experimental studies of Don Borghi, C. Giori, and A. Dall'Olio. Santilli studied the problem as we have also indicated in this paper. Thus, I quoted the authors (3, 4, 5, 6). So, I did not claim any priority on the results of other authors; I indicated the priorities of all their formulations in the framework of my personal elaboration using only biquaternions.

There is more. Let us consider paper [3]. Just five lines after starting the discussion in the paper (I repeat: five lines after. . . not five centuries after) I quote Reference 2 containing my articles published in *Physics Essays*. I think that when an author writes a text, he writes all the lines of this text. Correspondingly, when a reader reads this text, he must read and he must take seriously all the lines of the text; he cannot read all that he likes and avoid reading and considering all that is in disagreement with his thesis. In paper [3] in *Infinite Energy* I say that I am summarizing my results on BQM and I include Reference 2 just as a suggestion to the reader to see in detail and to deepen his understanding of all that I am discussing. If I indicate this several different times in the paper, it is intended that the lecture in Reference 2 is a prerequisite for the paper. This was, in fact, my aim, and it is the aim of any researcher writing his paper. If this were not so, a paper would be about a thousand pages long instead of a quick development.

So, let us follow my suggestion and review my articles in *Physics Essays*, and, in particular, my "A Generalization of Schrödinger's Equation Using. . .". Let us read from the page containing (48) to the page containing (73) where my generalized Schrödinger's equation is obtained using only biquaternions. As previously stated, I use Gisin's model and I show that my choice $S^{-1}= 1 - |u\rangle\langle u|$ is a particular case of Gisin's formulation. Before getting this result, I wrote:

"First of all, let us consider the important argument that we have outlined in the introduction of the present paper: the generalized Schrödinger's equation ***that we have obtained is a special case of Santilli Lie's admissible*** Schrödinger's equation."

And previously in the introduction (the second page) I say: "The generalization of Schrödinger's equation that we obtain by using biquaternions is precisely a special case of Santilli Lie's admissible Schrödinger's equation. We will outline in detail this result." So, are we not able to understand this text?

To confirm: in the references in my paper one finds Hadronic Mechanics is a result of the contributions of various authors (just as Santilli states), in particular Santilli. For a detailed exposition see R.M. Santilli's *Elements of Hadronic Mechanics* and I quote the preprint book that he sent to me.

All this concerns the generalization of Schrödinger's equation. Let us now consider previous Santilli studies on the formation of the neutron.

In my entire paper in *Physics Essays* I do not speak about the neutron, not even one time. Only the remaining six lines of the paper are devoted to this subject and I write textually:

"The generalization of Schrödinger's equation admits the possibility of bound-state formation. . . ***Santilli has discussed in detail the case of the formation of the neutron by*** (p, e^-)" and again his basic paper is quoted in this regard.

Again, are we not able to understand a text? I explicitly report that previously Santilli discussed the formation of the neutron on the basis of his generalized equation. Let us consider the references: ***I quote nine authors in addition to me. Four (with respect to nine) references included Santilli, regard Hadronic Mechanics and/or Santilli results.*** This paper is a demonstration about Santilli with regard to the problem of the equation and with regard to the problem of the formation of the neutron.

Let us go to my other paper, "On the Generalization of the Physical Laws by Biquaternions . . ." which was published in *Physics Essays*. Again, you see that in the references I quote five authors in addition to me: three of these references regard Hadronic Mechanics and Santilli in particular. I repeat that this paper is a demonstration about Santilli.

As I have discussed in detail in previous pages, in this paper I derive (3.10) used in *Infinite Energy*, that is the norm of the biquaternion Z′ with the basic relation $x'_\mu = b_\mu x_\mu$ I quote only three parts of this paper but the reader is encouraged to read the entire paper.

Read thirty lines after (29d) regarding the generalized norm of the biquaternion Z′ and the distinction between point-like and non-point-like particles; I wrote: "***More recently, starting with 1978, Santilli proposed a study of the Fermi Legacy and he suggested that a more adequate invariant should express the local functional dependence of light as well as the general inhomogeneity and anisotropy of material media. This author just suggested the invariant (23)***" that shows formal correspondence with the norm of the biquaternion Z′ given by us in (3.10) in *Infinite Energy*. The reader is also invited to read the paper and to verify the other times that the same concept was repeated.

This is if one considers only two of my various pages in *Physics Essays*. The same approach was also followed in the remaining papers. All this clearly shows that all of Santilli's arguments regarding plagiarism need to be rejected.

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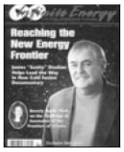

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